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MALIGNANCY AND EVOLUTION

THE SERPENT'S FANG

Essays in Biological Criticism

BY

MORLEY ROBERTS

The unexplained ordinary occurrence and the exceptional case are the sure clues to discovery.

SIR ARTHUR KEITH.

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PREFACE

It is a commonplace that any scheme of eclecticism meets with little favour from contending schools. Those who use such have few friends, since points of disagreement usually cause more resentment than the satisfaction given by points of agreement. If, then, I have in this book felt compelled to use Darwin and Lamarck when it seemed advisable to do so, and have rejected both when they could not be reconciled to certain later views, at least partly my own, this was done with eyes open to the probable results. Any hope that the selection of arguments from these two great naturalists might appease some opponents finally disappeared when the impossibility was seen of reconciling either with the newer school of Mendelian Darwinians or geneticists, whose arguments have failed to convince so many in spite of their admirable industry in constructing them. And when it became clear that at the basis of these newer doctrines of mutation there lay what may be called the "genes" of preformationism, or those ghosts of entelechies to be discovered in the conception of "mind" which has lately been so often used by many who find themselves unable to divorce religion from science, I felt it would indeed be much if anything to be argued on the other side would not be utterly ignored. If this has led me, no doubt rashly, to discuss in a late chapter the various disguises in which metaphysics has appeared in science, the fact will not surprise those who resent in silence, as they go on with their work, this later resurgence of obscurantism, which almost comes into line with the profound and catholic ignorance

of science displayed by such passionate initiates and neophytes in theologic lore as Mr. Belloc and Mr. Chesterton.

No doubt it will be said that the apparatus used is partly Lamarckian. This might possibly seem a bid for consideration on the part of Neo-Lamarckians. I have few delusions on this point. I have found in them a stern determination towards " direct proof," by experiment, of the inheritance of acquired modifications. It is not that I think these experiments can be ignored, for some have shaken the confidence of many who would utterly discredit them. But more is needed than this. I have therefore pointed out once again that in anatomy alone, in the construction of many of the internal organs, there can be found the clearest evidence of the continuous inheritance of forced functional modifications. This view was worked out with some completeness in 1918 and was reinforced in 1926 by further arguments led up to by the study of malignancy. The theory can be expressed shortly in the phrase " breakdown and repair." In 1920 I was led to lay some stress on the view that strictly analogous conditions could be seen in the evolution of social organisms, or States, as all social phenomena were strictly biological. When a state moved towards pathological conditions which were again patched up and repaired, here also breakdown and repair could be continuously observed in action. I am glad to note that, arguing from a different basis, C. M. Child in *The Physiological Foundations of Behaviour*, 1924, reached real biological conclusions as regards the organic behaviour of social States. But so far these conceptions seem to have received no attention from the Neo-Lamarckians, whose case they support very strongly, while such an apparatus in no way ignores Natural Selection or Darwin generally if we remember his employment of use and disuse. It may be remarked that no one when considering any of these problems in the light of social physiology and pathology should forget that

it is to Hobbes we owe the idea of the State as an organism or "animal" and that Herbert Spencer endeavoured to show how such a conception could be usefully employed.

The use made of what I have called Hostile Symbiosis, an independent development, perhaps, of Roux's "struggle of the parts," can be looked on either in a Darwinian or Lamarckian light. The partial autonomy of the tissues, which was recognized by Bichat, obviously leads to the conception that there is between them a continual conflict, though as a rule it is a subdued one, since a real revolt from settled or partially settled order must result in death, perhaps by malignancy itself. This conception again led to the formulation of a general theory of intra-organic immunization, a drama in which the ductless glands play the chief parts by their reactions to the secretions of all the others which thus lead to increased growth of the body and of the glands themselves through excitation of their qualitative and quantitative effects. This theory offers a definite machinery to account for growth in size, and also for the phenomena of overgrowth. That it largely negatives the doctrine of the survival value of variational accretions is true, but that particular doctrine has very narrow limits and can properly be applied only to some of the variations which are forced reparatory variations. The accumulation of purely casual variations of a speculative value is not now accepted by many. I see no reason why this view of intra-organic immunization should not be received by both Neo-Darwinians and Neo-Lamarckians, though I own to being unable to give any complete formula rendering it acceptable to geneticists.

What I have felt impelled to say of that which may be called "mentalism," or the doctrine of the "mind" as an effective factor in evolution, may have led me to speak with rashness of certain workers who have undoubtedly done good, and even great, work in their own special branches of science. It is, however, possible to regard all such work

with the highest respect, while criticizing certain errors or aberrations of speculation in questions which should be left, though without any hope of their solution, with the metaphysicians. The great underlying principle of scientific work is that Nature can be, and in time will be, explained, or at least put into intelligible order, without resort to any factor which partakes of mysticism. It assumes that the use of obscure metaphysical conceptions as regards phenomena now in process of pure scientific analysis is totally illegitimate. This is undoubtedly the case with "mind." Very little examination shows that those who fall into this error are invariably moved by religious or quasi-religious notions which they are reluctant to lose or to see strictly examined. They are therefore compelled to do all they possibly can to "rationalize" them. The orthodox in religion and the advocates of the old or the new vitalism, those who traffic in entelechies or in various forms of preformation, and those still later mentalists who in vain disavow any form of vitalism, obviously belong to the same order since they endeavour to preserve the notion of some director of growth, metabolism and co-ordination who, or which, can nullify at pleasure the laws of nature, already statistically determined, and those suspected to exist. There seems not a little reason to accuse the geneticists of similar scientific heresies, however vigorously many of them deny it. Some consolation, however, may be drawn from the fact that in their best experimental work they are every day laying more stress upon the internal and external factors of environment, and are thus undermining their own position. Some of them, indeed, begin to see this. If the characters attributed to hypothetic genes are merely fluctuant and unfixed variations the fact does not imply that genetics may not yet help us to understand how they can become settled characteristics. Whatever doubtful value genetics may have in pure science it can be shown to have value in breeding and in agriculture. This

very economic proof of its practical value strongly supports the conclusions that its phenomena are those which might be looked for in a trial ground of variations forced upon organisms as their protoplasmic and hormonic potentialities are variously and successively developed by the stresses and the stimuli of the whole environment, internal and external. The cytoplasm will at last come into its own, and it will be recognized that it can use and must use on bio-chemical lines the great bulk of the chromatin, though some of it is concerned with pure Mendelian or late unfixed characteristics. We still know very little of the complex machinery in the great city of the cell, and what it can do with its tools. Even the doctrine of specific protoplasms itself may presently fail when the chemistry of the nucleins and the cell's enzymes is really understood.

Studies of the probable course of evolution in some particular species as it has differentiated from the pre-specific genus to which it belongs have, so far as I can ascertain, been rarely used in biological investigation. It was the belief that such a brief and probably crude imaginative picture as that drawn of the viper in evolution might suggest methods of investigation, hitherto neglected, which led me to undertake such a rash adventure in a subject to which I came with a mind unmarked by prejudice, as I have never studied serpents, and also to make earlier studies in the evolution of the heart, stomach and uterus. But if the plain facts of zoology and gross anatomy have been neglected by the authorities in the fascination of experimental embryology and the like, it seems that pathology has fared even worse at the hands of biologists. They have utterly ignored it. And yet that it shows, over the whole of its enormous field of experiment, in conditions not liable to the errors of the bench, that the factors of disease and that temporary stage of comparatively easy action which we call health, are exactly the same, seems to

me indubitable. To learn how protoplasm works rightly and according to the laws of the factors concerned is the biologist's task. How can he better understand this than by seeing how and why it goes wrong? Would not John Hunter have agreed? And if I am right in suggesting that going wrong and getting the results patched up by repair are great factors in adaptational variations it is clear that the approach to and the recession from pathological states is a real process of epigenetic evolution by the process of continuous trial and error.

Although the underlying premisses of the arguments in this book deny particular validity to all schemes of tentative general explanation known as philosophy, I have not attempted in any way to replace them or to solve the insolvable. There is much better work to be done in this world than to chop logic at the bar of the Absolute. A real philosophy which is not merely a verbal forecast founded on ineradicable metaphysical prejudices must, like life itself, gradually evolve. It can no more be foretold than the final forms of inchoate organisms. I cannot, however, refrain from remarking that the arguments used and the conclusions reached at least permit us to believe, contrary to the opinions of orthodox biologists, that the individual struggles of organisms tend by heredity to influence the future of a race. The theories which attribute all progress, whatever progress may be, to unlinked accidents and deny that the gains, cerebral or other, of one generation may, with good fortune and continuity of effort, be transmitted, is I believe as false as it is in some ways depressing. Any reasonable attempt at proof that trial and error, effort and failure and renewed effort, however reflexly brought about, have cumulative results in the evolution of man and the organic world at large, should in some measure mitigate the curious resentment so often felt and bitterly expressed against those who find nothing foreseen or ordained in the subject matter of biology.

It is, it must be owned, a rash experiment for any one not a professed student to criticize the particular conclusions of those who have had the opportunity of devoting their whole energy to science. It has, however, been my good fortune to find many such who were aware that no one, however eminent, could possibly know everything and always be in the right. I have therefore some hope that even a professed herpetologist or ophiologist may discover not merely the errors into which such as myself must almost inevitably fall in a subject like that, let me say, of viperine evolution but may also find, here and there, something throwing a little light on processes which have not so far been analysed and yet seem of great importance in theory. I may say the same of any specialist on whose reserved ground I have rashly trespassed. It is, therefore, only proper for me to observe that all said in this book, however dogmatically stated in certain cases, should be regarded as mainly suggestive. It is difficult, indeed, to put any case in complicated subjects without employing what may be called the shorthand of apparent dogmatism. I shall regard it as a sufficient apology for these pages if they make but a few biologists reconsider some of their tentative conclusions, and thus have the good fortune to lead by suggestion to further work in the great science of living matter.

MORLEY ROBERTS.

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THE SERPENT'S FANG

CHAPTER ONE

The Viper on Exmoor

The Fang and Natural Selection—Acquired Characteristics—Breakdown and Repair—Darwin and Lamarck—The Viperine Fang—Causes of Variations—The Ophidian Skull—Anatomy of the Jaws—Origin of the Ophidia—Pathology and Evolution—Failure of Orthogenesis—Correlation and Co-ordination—The Movable Maxilla—Evolutionary Disease—Functional Stress—Immunity and Co-ordination—Evolutionary Disarticulation—Food and Function—Snake as a Whole—Function and Organization—Pains of Dentition—Rotating Maxilla—Evolutionary Fractures—Muscular Machinery—Fang Evolution—Dental Stresses—Morbid Origin of Poison Sac—Pathology, Sociology and Evolution—Disease and Construction—The Sac a Sinus—Analogous Poison Glands—Armed Poisonous Fish—"Idols of the Den"—Injury to the Germ—The Time Factor—Nature of Mutations.

The Fang and Natural Selection.—There are many vipers to be found on Exmoor and, as I share the feelings in regard to them expressed by a great naturalist who is no longer among us, I prefer to watch rather than to kill them. Of old my experience of living Viperines and Crotalines had not been small, and when I look back it seems a curious thing that it should have been left for a little English moorland viper to beget this chapter. Yet so it happened, for it was the sight of such a snake which led a friend of mine who has made his home in that natural wilderness, one of the few now left to us, to ask if I could explain the nature of its fangs. He wanted to know how and why they

became long, curved, hollow and moveable. For, though without any pretensions to be a zoologist or biologist, it seems that he had found himself unable to accept any theory as regards them founded upon ordinary Darwinian principles. That mechanism so complicated could arise by the accidental addition of accident to accident, each minute and spontaneous, was too much for an engineer to believe. He would as soon have attributed the modern steam engine to a similar set of causes. And although he had never formulated such a view for himself, at least not in words, his general way of thinking might easily have led him to the conclusion, which I had formulated long before, that there must be, by the very nature of things, a really general law of construction.

I do not propose to use here the rough suggestions I made to him about the serpent's fangs, although some time afterwards at my request he wrote them down for me with commendable accuracy. When the natural habits of critics are considered, and the almost insuperable difficulties in the way of an explanation, which insists on the descent, in certain conditions, of somatic modifications, being made acceptable to them, even though the argument be closely knit, how can they be expected to accept the rude and ragged pictorial history so rashly sketched out as we walked to and fro in a pinewood ? I, too, had once been a narrow Darwinian who believed that the prophet of Down had put the world and its natural history into my hand, and I knew how hard it was for the orthodox to yield a jot or tittle of ancient doctrine, even though many are now well aware that it is the orthodox who have maimed and mangled Darwin. Do not the orthodox always pervert their origin ? We see it in religion, and as for some biology is a religion it may become a rigid creed and beget hostilities and strange distortions of the spirit.

Acquired Characteristics.—This talk on Exmoor, far from books and the haunts of learned men, showed me once

again how much we may sometimes owe to the casual questioning of the curious and interested man wholly outside the circle of science. We may learn more from them than from many professors. For when seeking to explain we very often learn to understand. Till that day I had never set any kind of logical apparatus to grind out a theory or a rude hypothesis as regards the subject dealt with in this chapter, even though I regarded the serpent's fang as a subject to be dealt with some day on the lines hinted at, but not quite truly laid down, by Lamarck, while Darwin might be called in to aid in the delicate operation which we call "explaining." Though my curious companion of the woods and stream of Simonsbath knew little or nothing of the distinguished and neglected Frenchman, he had no prejudice against the inheritance of acquired, or altered, somatic characters or characteristics, provided they led to something which looked like an explanation, and here an explanation was clearly something helping him to class these phenomena with constructions of which he knew much. So he welcomed gladly any scheme, however crude, which put aside minute "spontaneous" advantageous variations building a complicated instrument by nameless karyokinetic chances and turned, not unwillingly, to the school of the unorthodox, which now at last begins to come into its own. Let me say that what he knew of genes and their magic capacities was not enough to do him any intellectual damage.

Breakdown and Repair.—It was after this talk was over and past that it began to bear some fruit, or at least to bud, for when considering it again after a busy year I began to feel that if the crude Darwinian view was denied in favour of a modified Lamarckian scheme, even that was itself incomplete and must be added to. But it seemed to me that when two such opposite views are held strongly by learned adherents, the one which commends itself to the technically untaught but truly intelligent man is to

be preferred until a better, or perhaps joint, explanation can be offered. This may not be a biological judgment but it seems one likely to commend itself to a judicial mind. It is on some such grounds as these that the legal intellect distrusts experts when they differ from each other and come into conflict with a body of circumstantial evidence. It seemed to me then that if to what these great naturalists have given us there was added the simple suggestion that fitting variations arose naturally by the properties and reactions of living protoplasm, through the action of repair following breakdown or partial breakdown, that some such explanation could be offered. For very little consideration will show that such a source of variation is a great source of tentative construction not merely in mechanical things but in every branch of human knowledge. Hypothesis and theory themselves, nay the very theory of evolution itself with all its failures of explanation and the fresh variations and suggestions of workers, declare it to be a general law.

Darwin and Lamarck.—Though loath to prolong this prologue I cannot help remarking that nowadays it requires less courage to attack the accepted doctrine than it did some years ago when the bishops and high priests of the Orthodox Church blew trumpets of triumph about the tomb of Weismann, who had settled everything. Nowadays there are few trumpets, the shrine is almost desolate, and very visibly there are cracks even in the creeds. This has its important side, for it has given occasion for some biologists and zoologists, with an ineradicable tendency to dilute science with religion, to proclaim with new fervour the misty doctrine of vitalism and the even mistier doctrine to which Dr. Haldane has lent his authority, and to allege that no explanation of evolution can ever be complete without an unscientific top-story in which to lodge some directing and co-ordinating deity or that faint god-like ghost known as an entelechy. And yet such obscurantism,

bringing as it must a strong reaction against it, may afford a better chance than there once was for the restitution of both Lamarck and Darwin to their proper chairs of honour. Many now recognize that they were much nearer together than Darwin knew, and later some reasons may be suggested to show why Darwin neglected his great predecessor. Here I propose to use as much of both as seems necessary, though to their apparatus must be added the still unaccepted doctrine of breakdown and repair. After all to do so is but to draw conclusions they might easily have drawn for themselves, for both were much concerned with disturbing conditions which, as a very little consideration would have shown, were morbid, when not lethal, influences. That Darwin, with his ever increasing addiction to use and disuse, was more Lamarckian than he ever acknowledged, or perhaps knew, is nowadays quite clear. And we cannot be sure that Lamarck, if he had been confronted with the principle of Natural Selection might not have said, " Why, I always took that for granted and saw no need to give it a special name," though he would have to admit that Darwin won a great victory in the very field in which he himself had been defeated.

The Viperine Fang.—It is obvious that when we look at the poison fangs of snakes we are at once in peculiar difficulties. A minute variation of a chance order must be of " survival value." How could such be of advantage ? It will be seen, when we come to deal with vespiform moths, that this is a monstrous obstacle. For when we talk of survival value and Natural Selection as regards such moths we are really referring to the perceptions of birds, insects, and lizards, not to some voracious but nervous General Principle with microscopic eyes, which can detect minute variations with unfailing accuracy and instantly bolt in terror. In the snakes we have to explain not an apparently single variation but an apparatus which has at least four main obstacles to our grasping its evolution.

In the Viperines and Crotalines we have (1) the great overgrowth of the canine teeth, (2) their grooved or tubular character, (3) the maxillary hinges, and (4) the poison and the poison sac. We must be allowed to ask those who believe in the orthodox view in what part of this marvellously correlated mechanism its original variation occurred. But to ask it shows at once that the very question is ridiculous. To reply to it seriously would be to neglect the obvious fact that the original difficulty of imagining a single minute variation was not merely increased fourfold but to an almost infinite progression, since we should have to infer that every other variation was correlated with the original minute variation. A mathematician might perhaps work out the chances against the evolution of a poison fang on these lines, but it is a task that most would refuse not only to undertake but even to contemplate.

The Causes of Variations.—If, then, it seems impossible to use any crude Darwinism here, can we employ any equally crude doctrine of Lamarck? However much we may believe in descent with modifications we must have some notion as to how these modifications arose, and Lamarck's "need," however we interpret it and with what glosses, cannot satisfy us. It is plain that both these zoologists fell down on, and in reality gave up, the problem of how variations came about. Darwin frankly acknowledged as much and Lamarck never, so far as I am aware, explained why use and disuse acted as they did. He simply, and at his time perhaps rightly, assumed what seemed to him an obvious fact. But we are further advanced now. We want to know the machinery, and machinery must be intelligible, not mystical, and must not import more difficulties than it solves, after the manner of those who employ conceptions more difficult to understand than the original problem. I am aware of no suggestion in later work which seems more promising as regards real results than the addition to Darwin and Lamarck of

the principle of breakdown and repair, a method of evolutionary trial and error common to all construction.

The Ophidian Skull.—We may, then, now attempt to apply the organon, so roughly outlined, to some of the problems connected with ophidian dentition, especially with the dentition of the Viperines and Crotalines. Not being a professed ophiologist or comparative anatomist need deter none from considering these problems. The authorities are many, and so far as the gross anatomical construction of snakes is concerned there are no difficulties or great obscurities to be found. In saying so much I do not mean to minimize the obscurities which are probably still to be found in the development of any vertebrate skull, and in the realm of comparative anatomy. So far as I am aware this was worked out as regards the adder by Rathke (1839) in his classical paper which, for sufficient reasons, I have not read. The first obvious feature of the ophidian skull of importance here is the extreme mobility of the osseous parts employed in deglutition. "The total absence of both zygomatic arcades, the non-union directly of the palatine with the vomer, the laxity of the union of the pre-maxilla and the maxilla, and especially the moveable condition not only of the quadrate but also of the squamosal . . . from which it is suspended, all give an excessive mobility to the facial part of the skull." (Mivart.) The mandibular rami are not joined but united by soft extensible ligaments. This is the condition of all the Eurystomata, or wide-mouthed snakes. In the narrow-mouthed, or Angiostomata, the rami of the mandibles are closely united by ligament and the maxilla is fixed to the skull. Those interested in further anatomical details can find them in any competent text-book of zoology.

Anatomy of the Jaws.—In reptiles generally the mandible is articulated by a single occipital condyle moving in the quadrate bone. This articulation survives in the Lacertilia descended from the earlier articulated ancestor which

existed before the Tertiary period in which true snakes appear. There are a very large number of variations in the various genera but, except in the ophidia, articulation, however varied, is a constant feature. While the palatal and connected bones are firmly fixed to the upper skull in all reptilians, in all fishes but the Dipnoi (lung fish) this is not the case. In fish the jaws are hung posteriorly upon the skull by the hyomandibular cartilage, which practically disappears in the Dipnoi, and anteriorly by the ethmo-palatine cartilage. It is inferred that the lung fish are thus related to the land vertebrates by their possession of such common ancestral characteristics. In reptiles the hyomandibular cartilage disappears as a ligament, as it does in all mammals and birds, in which it becomes the auditory ossicle, and the maxilla is fused with the skull, thus being immoveable. The ethmo-palatal ligament, so far as the upper jaw is concerned, also disappears in all reptiles, but in the ophidia the lower part remains and serves the part of a loose articulation as there is no condyle. Perhaps this and the absence of bone symphysis in the mandibles are their chief differences from other reptiles. In the narrow-mouthed snakes (Angiostomata) the rami of the mandible are closely united by comparatively inextensible ligament: in the wide-mouthed serpents the rami are loosely united by very extensible elastic connective tissue, analogous with that of the *ligamenta nuchæ*, and the *ligamenta subflava* of the vertebral column of mammals.

Origin of the Ophidia.—I am not aware whether ophiologists find it possible to assign the likely origin of the ophidian, as distinguishable from the primitive reptile, to any particular stage of reptiliform animals, though snakes and lizards can be traced back to the Cretaceous period, when reptiles were represented by two sub-orders of aquatic reptiles, Dolichosauria and Pythenomorpha, which appear in some respects to be intermediate between living Lacer-

tilia and Ophidia. But these are only found in Tertiary and Recent deposits. If their origin cannot be placed very far back in generic history this affords some grounds for supposing that the non-articulated suspended mandible of the ophidians is not so primitive as might be imagined, but that it is a reversion to a pre-reptilian character. That such cases happen is well known. The irreversibility of evolution has its limits. A condyle may appear in evolution as it has appeared in reptiles and may be maintained by continuous function. That it can be duplicated we know and, if disused, it might wholly disappear and be replaced by ancestral ligament. It is not stretching language beyond its elastic limits to call the result seen in snakes an evolutionary dislocation of the pre-ophidian jaws. In any case we need some explanation as to why true reptiles such as the ophidia lack final specialized articulation of the mandible and also of the difference in mandibular connections between the Angiostomata and Eurystomata. If I put this down to the principle of use and disuse, to new forced function leading finally to heritable conditions, the suggestion is no doubt Lamarckian and therefore heretical. There is, perhaps, no grave reason for disbelieving in a narrow Darwinian view, provided time enough be allowed for the changes, if we are able to believe in the possibility of definitely chance variations occurring and accumulating in a right line. It is in the further developments of which I have spoken that there can be found reasons for refusing to assign these results to such a pure factor as Natural Selection by itself. And in the same way I should refuse to employ it in one of the most interesting lizards now found, the *Scincus officinalis* of Egypt, which has been modified for, and by, a life largely lived among the sands of the Egyptian deserts. It is a true lizard, but the unlearned observer at once characterizes it as a snake with small legs. That it became what it is now can be looked on as a later case of convergent evolution to the snake form and from it we can,

without much rashness, assign all snakes to an origin in sandy deserts in which legs and feet were of small use as organs of progression. That lizards find this out we may infer from the fact that some of them in Australia and South Western America when alarmed or in haste use their hind legs only. I refrain from drawing any speculative conclusions as to the kangaroo or the possible further evolution of some of the African Cervidæ, which often stand on their hind legs as they feed on tall bushes. The environmental effect of use and disuse in such cases as that of the *Scincus* is reinforced by the snake-like or eel-like appearance of such amphibia as the Siren and Amphiuma. (T. Barbour.)

Pathology and Evolution.—It is now some years since I first suggested that pathological states may be, and indeed must be, part of evolutionary mechanism. I say must be, for very little consideration shows that any variation whatever is a disturbance of settled order and is in fact a disorder. This can be argued on pure general principles and illustrated by the fertile analogies which may be drawn from all the sciences, including sociology, in which evolutionary methods are clearly in action at all times. Such problems I have dealt with elsewhere in *Malignancy and Evolution.* But independent of such arguments we find sufficient proof in biology alone of the inter-dependence of pathology and physiology. I shall not now repeat work done on these lines in which I use, as cumulative proof, the origin of the heart, the external testes, the uterus and the stomach, but may add here that only recently Leriche and Policard (1926) have demonstrated in France that the very formation of bone is carried out through a complex series of pathological states, thus adding some force to a previous suggestion of my own that bone could be looked on as senile cartilage or as cartilage so stressed as to take on disordered changes before settling down, under the direction, if it may be put so, of those

fibrocytes which become osteoblasts in the presence of calcium accumulations. If these views of pathological states in evolution have any foundation it does not seem extravagant to believe in the necessity of painful and disordered evolutionary processes. What shall be said of the phenomena of child-birth among human beings at the present time ? We have here generally a forced and painful process often leading to death. This, as I have frequently observed, occurs also in domestic animals which, as Darwin showed, are more liable to variation, which may be interpreted as lack of stability and disorder consequent on such a lack. For disorder of any kind is variation : even definite disease, which may have evolutionary effects if recovered from. I have therefore no difficulty in imagining that what I have called an evolutionary dislocation may have come about during a comparatively late period of time, a conclusion somewhat reinforced by the fact that even now snakes sometimes die with their prey impacted in the jaws.

The Failure of Orthogenesis.—However the present ophidian skull is explained, and by explanation is meant the suggestion of likely and not impossible stages, we are far from any explanation of the fangs, whether fixed as in the venomous Colubrines, or moveable as in the Viperines and Crotalines. How comes it about that the fang is venomous at all, and why is it fixed in one case to a rigid, and in the other moveable on a shortened, maxillary ? To attribute the skull form, the fang and the poison sac, a closely correlated nexus of changes, so closely correlated as to give a shadow of a reason for orthogenesis, to accidental spontaneous variations seems impossible. And yet this has been a determined line of evolution which can be explained if we turn to environmental stresses continued generation after generation, while at the same time using Natural Selection so far as to see that fatal accidents must have removed many ophidiforms with too much rigidity

for them to change without disaster. Such a Lamarckian hypothesis, modified by what has been learnt since Lamarck's time, actually shows why there is a continuous related set of changes while it does away with any necessity to call on orthogenesis. For it gives a reasonable explanation of what made some hasty theorists support that as the only possible explanation. Arguing on these lines we are forced back on definitely caused inherited modifications, for if we can suggest a likely set of causes, or antecedent environmental conditions, we may get something like a comprehensible scheme of the evolution of the ophidian skull, the fangs, whether venomous or not, their curvature, their grooves or canaliculate form, the poison sac, small or immensely extended. These seen as one extraordinary set of allied machines must surely demand a mystic agent, or a common-sense view not impossible to swallow by, shall I say, my engineering friend on Exmoor. I shall, then, work on the line that stress, breakdown and repair with consequent regularization or physiologization, and the inheritance of forced changes, however apparently disordered the process, can alone give us any comprehensible continuous scheme of ophidian, and especially of viperine and crotaline, construction. For then there is no need to call on any chance variations but those due to the environment. We have a definitely linked mechanical process. Those who know Darwin best will not say he is here discarded, for it is only his modern followers who have entered his tomb and robbed him of the weapons of use and disuse. It is true that he never resolved the contradictions between them and Natural Selection, but such contradictions are common.

Correlation and Co-ordination.—It is clear that some such exposition of the difficulties as is here attempted was necessary in order to clear the ground of the common popular supposition that a good and clear Darwinian explanation could easily be supplied if ophiologists chose

to give it, perhaps when they had time. However this may be, and I have found biologists who practically said as much, it is certain that many who are not biologists but are still believers in orthodox theories, regard the problem as more or less a minor point, since Darwin and Mendel have now settled everything. This, however, seems to be a mistake, as a general view shows. To discuss the various points separately and not to regard the whole complex co-ordinated machinery as it works more or less perfectly would be vain. And here it may be remarked that co-ordination, which Haldane holds to be impossible of explanation without the illicit interpolation of some deity in the vast major premiss of the universe, is the main object of scientific research. To deny this is to stultify all scientific work. In a later chapter I hope to deal with the more delicate modern forms of obscurantism, and shall not here labour the point.

The Moveable Maxilla.—To discuss then the origin of the fangs themselves without reference to the whole organism and its history and environment would be idle. We shall not thus explain how it came about that the colubrine poisonous snakes have fixed and the viperines and crotalines moveable fangs. For here we come to a remarkable fact which is not known to all popular writers on serpents. The moveable fangs of the two families of the sub-order *Ophidii Viperiformes*, that is of vipers and rattlesnakes, are not moved in a socket. It is the upper maxillary bone itself, which is very short, which moves, pushed by the ectopterygoid on its loose articulation with the pre-frontal when the fang is erected or laid backwards. The tooth is firmly united or ankylosed with the maxillary. This is always the case in existing species of the Lacertilia, though in some extinct species sockets are found. In many reptiles the ankylosis of pleurodont or acrodont teeth is not of such a character as to prevent their frequent loss. There can be found, in the tissue on the fang's

posterior root surface, embryonic teeth which replace the lost adult fangs of these snakes.

Dental Ankylosis.—This dental ankylosis, or complete union of an epithelial product with a mesodermal structure such as bone, is seen in others than reptiles. I take it to be a "cured" result of the forced loosening of the teeth, such as is seen among fishes. It occurs in Sphenodon (*Sphenodon Tuatara*), that peculiar lizard-like surviving member of the generally extinct Rhyncocephalia, which is found in New Zealand. The extinct forms include the earliest known reptile (Palæohatteria). On the lines of the analysis suggested before we must infer that such an ankylosis may be looked on as a zygosis dependent on a morbid condition which has undergone prolonged evolutionary repair. Those who have not yet grasped any idea of the part played by pathology in evolution will no doubt regard this as a rash speculation. But they may at least be asked to grant that it may have had some effect or, at least, that the suggestion may be made the subject of inquiry.

Evolutionary Disease.—If any variation from normal form can be looked on as disorder since it implies correlated disturbances of settled order and readjustments, it seems clear that whenever it occurs this fusion of distinct tissues with the loss and disappearance of dental periosteum is the result of what may well be called evolutionary disease. It is at the least a condition which requires explanation. To look on the gradual disappearance of periosteum as due to minute "spontaneous" advantageous diminutions is obviously to run a theory into the ground. We must know how these minute variations were caused. Its loss means the forced substitution of the nutritional dental membrane by actual bone. A highly evolved epithelial product looses its historic establishment and is forced into the closest partnership with mesodermal tissue. If the Darwinian doctrine so far as minute advantageous

variations fails, such a change cannot be attributed to large Mendelian factors. We might as well explain thus the beginning of the coloration of some snakes and lizards in which excretional products, such as guanin and carbonate of lime, play a great rôle. So uric acid acts in some butterflies. It follows that these changes are due to continuous stresses, that is, to abnormal conditions leading to periosteal atrophy and to a very unusual zygosis of alien tissues, not to such a natural zygosis as is seen in the symphysis of the embryonic rami of the mandible in most animals. If these continuous stresses led gradually through this disorder of alveolar tissues to ankylosis as a static physiologized result by the functional diversion of energy in embryo and during growth, we can say that such minute variations were so far finally advantageous. At the same time it may be added that they may not have been minute in the sense that they were not reflected in the animal's nervous system as painful. As I must frequently refer to sociology as a lamp, we may well ponder over the fact that in the evolution of states there are frequent periods of prolonged social discomfort finally resulting in order and some physiological stability. These processes imply diversions and abstraction of social energy, and may, as we know, be accompanied by acute crises. But what is done in one generation is carried over to those that follow as new social functions create new structure or reform the old. This may be stated as social Lamarckism, though it is to Herbert Spencer that we owe the first clear recognition of social evolution as a biological process.

Functional Stress.—From what is said above it must, temporarily at least, be inferred that such alterations as those referred to arose from successive and trying functional stresses. In some, or indeed in most, such stresses were met with in other ways and common dental organization was maintained. An easily swallowed dietary would obviate such stresses. A capacity for exploring

a wide range would help. But in Ophidians the range is small and in the slow-moving vipers and rattlesnakes it is usually very limited. It is a commonly held view that this slowness is due to their deadly protection, but I hope to suggest some reason for believing that on the contrary it was to their very slowness and their small range that they owe the defensive and offensive apparatus. Thus habit, ecological conditions, overstress and consequent disordered variation, must on these lines be taken to have resulted in the inheritance of somatic variations occurring in a group, exposed to like conditions, consisting of males and females with the same habits and the same difficulties to meet. Elsewhere I have accentuated the probability recognized by Lamarck himself, that it is with groups only that such a Lamarckian doctrine can hold good, while at the same time stressing the fact that nothing is directly inherited except the changes in energy direction, and in the chemical constitution of the sperm and ova. Without attempting to touch on primitive or embryonic cytology we have to note that there may be and clearly have been from time to time new acquisitions of various catalysts, such as iron, forced on the organisms concerned by a changing environment. I forbear to speculate on the question as to whether such forced acquisitions had not temporary pathological results.

Immunity and Co-ordination.—If so much had to be said as preliminary to any rough sketch of the probable conditions which led to the construction of the ophidian skull and further to the evolution of the dangerous apparatus of the venomous serpents, we may now go on to consider very shortly the question of the necessary co-ordination of the animal and its weapons. It is true we know very little of co-ordination. But who but those with a concealed theological or semi-theological bias will deny that its comprehension is now one of the chief tasks set before men of science? They do not shirk its difficulties and

follow Haldane into religious metaphysics which seems but a way of eluding work. They are, if indeed endowed with a scientific mind, not in so great a hurry as to imagine that what they now begin to understand must be understood at once, or that resort must be made to religion and faith. It happens, indeed, that in the last few years very great advances have been made in the comprehension of co-ordination. This can be denied by none who have viewed without prejudice the progress of endocrinology. I trust that it will not be ascribed to rash over-confidence if it is said that the vital connection between nutrition and immunity and the contrary, sub-hostile, action of the ductless glands upon others, leading to a general theory of intraorganic immunity, which I have suggested and defended elsewhere, reinforce this view. It is, unhappily, due to the almost forced separation of the sciences in the present abundance of uncoordinated facts, that immunology has been left almost entirely to bacteriologists, who are not necessarily advanced physiologists, and therefore the range of the conception of immunity has never been properly recognized. It is in truth a great factor in that co-ordination which we are now bidden to believe beyond human wits to understand and above the power of protoplasm to construct. Every time we discern that one variation, however small it may seem, necessarily implies a change of cellular and tissue relations, we advance in the understanding of co-ordination. And it would be gross ingratitude to the powerful work of Pavlov if we neglected to acknowledge that the extension of the simple reflex into a real working system of conditioned reflexes helps us amazingly to understand in what way cerebral co-ordination occurs, and can produce reflex motor-reactions in strict relationship with function and consequent construction. It is thinking on these lines which has induced some American psychologists to discard consciousness as anything but a verbal delusion. To Pavlov we may owe

the final destruction of that ghostly reflex figment to which so many appeal. And it may be remarked that any study, even the humblest, such as is undertaken here, may make, if properly understood, some slight addition to the knowledge of co-ordination. It can be said that a thorough recognition of the relations of any organism to the stresses of the environment with pathological or quasi-pathological results, which are repaired and renew altered function, gives far more than a mere hint as to how co-ordination of all kinds arises and is maintained, unless the results are lethal. Any student of sociology with some knowledge of biology must see this in his own particular subject as he inspects the result of social stresses and the action of repair, legislative or customary.

Evolutionary Disarticulation.—To return here to the ophidian skull, we may say that what we want to know is how the reptilian skull of the ancestors of the ophidia changed and established itself as what we see it. There seem undoubtedly to have been some reversionary changes, though it is very generally, and usually truly, stated that true reversion does not occur. This is Dollo's Law of the irreversibility of evolution. But as we see real articulation of the mandible in reptiles, and in their ancestors, it seems highly unlikely that the ophidiform, or lacertiform ophidian ancestor did not possess this feature. And true evolved articulation postulates dislocation as a very serious accident indeed. But what as a fact do we see in the snake, or certainly in the Eurystomata? What happens when the reptile, such as the boa or python or even the small *Dasypeltis*, or egg-eating snake, swallows what is on any properly articulated system of jaws, clearly too large for it? There is as a matter of fact a very extreme reversion towards the suspensional system of the fishes, though the anatomy of these is far more complicated, not having been simplified. There is a disarticulation of the mandible from its place in the

quadrate and it is then suspended to that bone by tissues homologous with the lower strands of the hyomandibular cartilage. There tissues are very elastic : they seem indeed to be very little fibrous and are mainly composed of elastic tissue such as is found in the ligaments of the mammalian vertebral column as suggested previously. In some respects they appear to possess a rhythmic power of contraction, thus approximating to muscle. But now the quadrate, attached to the squamosal, a long bone also loosely suspended by like tissues, is also pulled away from its close proximity to that bone, thus greatly enlarging the gape. In the pythons there is no moveable pterygoid, but in the viperines and crotalines this and the ectopterygoid move and thrust at the moveable short maxillary in which the fang is ankylosed, turning it on its transverse axis and erecting the fang. In cases where the prey is excessively large there are not only these functional disarticulations, or evolutionary dislocations, but one which is even more suggestive of a pathological origin. For it is found, and I learn this can be seen without difficulty in captive pythons, that there is an actual dislocation of the occipital condyle of the atlas with a necessary concomitant stretching of the spinal cord. How far it is possible to carry systematists and the more orthodox biologists into a region of thought in which these phenomena are to be looked on as the result of forced variations of a highly dangerous and pathological type, experience forbids me to speculate. But these, however roughly and diagrammatically put down, are the facts and neither spontaneous variations, that is, those to which we can assign no cause, nor mutations can afford any reasonable explanation of the way they became in most cases functionally physiologized. I say in most cases, for as was before remarked, even yet snakes die with prey too large to swallow impacted in the jaws. Natural Selection, in the lethal sense, of course plays its part, but all these variations follow in a sort of orthogenetic

line, not because they are foreseen or ordained by some entelechy but because of the hereditary continuance of forced dangerous function. I shall refrain from calling this view by any name, but we may at least offer Lamarck a little incense if we interpret his "need" (*besoin*) in this place as hunger in snakes of a small ecological range with slowness of locomotion and a limited supply of suitable food. It seems clear that any python with a reasonable endowment of python brain would much prefer sufficient rabbits or agoutis to a large horned goat. Thus again we may note that the main body of venomous snakes are not slow because they are venomous but are venomous because their forced habits have given rise not only to their enormous guttural gape but also, as I propose to show, to their offensive armament as well.

Food and Function.—The whole way in which species live must be considered. Some smaller angiostomatous snakes live on such small prey as their general liveliness enables them to catch. It is difficult for those who have not seen some of these snakes in their native surroundings to credit the swiftness with which they can cover the ground. In Texas I was accustomed to see snakes, which I regret not being able to give a name to, which were of light silvery appearance and lived in the holes and crevices of out-cropping rocks. They lay about in dozens in the sun, but when I rode within fifty yards of them they fairly bounded across the prairie and disappeared in a moment. The rattlesnakes on the contrary were so slow that the smaller kind which lived on the open prairie fell an easy prey to a stockwhip, as we were acccustomed to kill them on account of the horses, cattle and sheep. Theirs was a very small range while the silvery non-venomous snake could easily and rapidly move great distances. It is not travelling out of the record to assume that the early reptiliform of Cretaceous times which finally gave rise to the Ophidia and Lacertilia was angiostomatous. Those who

refer with consideration and respect to the environment will scarcely hesitate to assume that the later division into small and large mouthed snakes depended greatly on the nature and choice of food, thus assisting the process of division into species. Now we have to ask what happened to those slower snakes which found some secular decrease of their normal food and an abundance of smaller animals, reptiles and batrachians which flourished, perhaps because the early ophidian found them hard to swallow. If the prime solutions of this are what I believe, the consequent co-ordinated problems may be partially cleared up, or at least made ready for further consideration by unprejudiced zoologists.

The Snake as a Whole.—To those who consider that the complications before us, when we look on the snake as a whole and do not blind ourselves by isolating each character and attributing it to undirected Natural Selection or to the accumulation of Mendelian genetic flukes, are such as to make solution on more general lines impossible, it may be replied that it is better and far more scientific to envisage all the difficulties of a truly mechanical explanation than to slide off into the realms of chance. The real conflict between the hostile schools depends on logical questions. Is it a logical process, or a semi-theological one, to refuse to exhaust the possibilities of practical explanation and to use *a priori* methods of a highly speculative character which leave some, not incapable of thinking, absolutely cold? The answer to me seems obvious. The difficulties cannot be evaded, even if at the best a sketch of what must have happened in the long period of ophidian evolution seems nothing more than a rough diagram.

Function and Organization.—It may, then, be taken as at least a temporary hypothesis that the loose organization of the ophidian skull is largely due to age-long attempts of snakes to swallow prey which put a heavy set of stresses on their ancestral apparatus. That methods of feeding

end in changes is believed by many zoologists. Those who have grasped the doctrine of hormonic descent need not boggle at the suggestion that forced environmental changes imply concomitant hormonic and constructive changes. Present function is at the root of all visible morphallaxis, but past function survives in the potentialities of fresh molecular arrangements or patterns, again and again made into embryonic mechanism. We may note that all tissues that rapidly renew themselves from an undifferentiated zone, as the epithelia and connective tissues, retain their ancient possibilities, as we see in metaplasias and heteromorphoses. Thus columnar epithelium may revert under stress and become squamous. A tissue may revert and go on again in a prosoplasia, a tissue further advanced than it was before it changed. These are commonplaces of morbid histology, but they may have value in general evolution if they show that the organism is a function of the past work it did which changed, slowly or quickly, the hereditary directors of growth. Following Lamarck I suggested earlier that such inheritance would only be very likely if both the males and females were exposed in groups to like conditions. Here, however, we are more concerned with the machinery of variation than that of descent. So we shall see when dealing with ophidian dentition that, if it looks far more logical to assume temporarily that a new forced dietary set up new conditions in the general metabolism of the skull and by use and disuse disarticulated the jaws and left them freely attached by elastic tissue, we may apply the same organon to the short moveable viperine and crotaline maxilla, to the functions of the pterogoid and ectopterogoid, the fang, the canaliculation of the fang, and to the actual poison sac itself. Thus we may possibly come to understand, however dimly, a great and obscure problem in co-ordination.

The Pains of Dentition.—Earlier it was suggested that the ankylosis of the fang was a final healed result of morbid

conditions set up by abnormal stress. This ankylosis is found elsewhere, but in many cases stress seems to have induced not the fixing but the continual looseness of the teeth. This is seen in many fishes, which continually shed fresh crops of teeth. So do some reptiles, such as crocodiles and alligators. The ophidian fang is not shed regularly, but when it is shed or broken it at once begins to be replaced by a new ossicle which also becomes ankylosed in its turn. This repeated ankylosis reinforces the view that what looks clearly in its origin a morbid process gets so shortened up during evolution that it may be mistaken for a phenomenon to which no morbidity can be attached. But even in human beings it is clear that the early stages of dentition remain real morbid processes, frequently accompanied by pain and constitutional disturbances. The inference that what we call physiology is vitally connected with pathology seems as irresistibly suggested by these facts as it is by the dangerous and painful phenomena of child-birth in women referred to previously.

The Rotating Maxilla.—It is possible that the different phenomena presented by the Viperines and Crotalines as compared with the Colubrines may be of some assistance in thinking of these problems. In the Colubrines we observe that stresses similar to those affecting the others have resulted in a poison fang but not in a rotating maxilla enabling the fang to be erected or laid backwards and so disengaged from the prey, for though in some colubrines the maxilla is comparatively short it is still firmly attached to the body of the skull. The argument may be obscure but so is the subject, as it seems to have been so little taken into consideration, but may we not rely on the generally accepted doctrine of re-enforcement and re-arrangement of structure under great but not breaking stresses and its failure under those which are excessive? This is in fact Wolff's Law adapted to heredity. There is no difficulty in assuming that the early maxilla of the colubrine ancestors

actually responded to that law and was strengthened, while in the erectile fanged snakes the loosely attached bones gave way to secular stress. When we see the salient facts of the disarticulation of the mandible and that of the occipital condyle from the atlas, it is not hard to picture the final detachment of the viperine and crotaline maxilla from its evolutionary neighbours, though in some cases this moveable maxilla has a loose articulation with the pre-frontal. For any looseness of attachment such as was the case with early reptiles and is common now in fishes, their near relations, would be accentuated by such use. And here we may say that its "morbid" detachment would have led to nutritional changes as the whole environment of the maxilla when free to move would have changed. Naturally it is easy enough to deny that these changes were as a matter of fact morbid in origin. We have only to imagine that this rude and brutal sketch of an evolutionary process is utterly false and that happy occasional slight variations, attributable to the magic of karyokinesis and wholly detached from environmental conditions, or large and beautiful Mendelian play among the genes, brought the results about easily, to discard the whole notion as fantastic. But whether it is more fantastic to imagine and invent causes which are really final causes in disguise and not proximate and effective antecedents I shall leave logicians to determine.

The development in the above paragraph of Wolff's Law, which properly speaking applies to somatic construction in answer to stress, into a general law governing what may be called the historic individual, will meet with little approval. The implication is that somatic modifications on any obvious line of development are necessarily inherited if the stressing environment and the resultant functions remain in action. I have, however, phrased it in this way to bring into greater relief the facts of continuous construction on definite lines which, to those not devoted to the forms of

speculative theory now in the ascendant, suggest irresistibly that the final cumulative result at any stage, when "pure" or divorced from accident, is wholly the result of past function. I shall have occasion in a later chapter to discuss very shortly the liberties I have apparently taken with Wolff, a man who did far greater work than he seems to have known, or than is generally recognized.

Evolutionary Fractures.—If Keith is as right as I think him in calling a joint an evolutionary fracture, we may look on all these maxillary changes as a set of fractures completely healed with functional separation, while in the colubrines there was an actual functional strengthening of the attachments which gave way in the viperines.

The Muscular Machinery.—How then did the maxilla, when it finally became detached from effective bone control, come to have muscular machinery to move it? This is a minor question but with what we know of the migration of muscle and its development under rhythmic stress, it should be solved without much difficulty. That the loosened pterogoid should be able to move by a muscular thrust against the transversum or erecto-pterygoid, so restoring the maxillary to its evolutionary place and therefore again erecting the fang, is not difficult to understand, since such action and such positions may be looked on as a temporary return to normality such as we see in the colubrines where no anatomical freeing of the bone took place. These suggestions, only proper to be worked out thoroughly by experts in ophidian anatomy who feel free to consider the group descent of forced somatic modifications, should not be repugnant to palæontologists, who seem not to have been so greatly subdued to current orthodoxy. That it will not commend itself to most zoologists I am well aware, but after all these changes occurred, and should be accounted for mechanically, not by magic or by some supra-sensible source of co-ordination.

Fang Evolution.—There are yet other questions to be asked. How did the overgrown venomous fang of the *Ophidii Viperiformes* become curved, canaliculate and a vehicle for poison, and what was the origin of the poison sac ? It seems clear that granting the evolutionary osseous looseness of construction in the vipers that the first stage must have been due to the overgrowth of the fang itself. Very little is known of heterogonic differential growth ratios, which J. S. Huxley has done so much to work out in deer and in the calling crab. In the viper it looks as if this differential growth had been at work, as it has in the case of antler growth, in the overgrowth of tusks in *Babirussa alfurus* and the extinct sabre-toothed tiger. Overgrowth, as I have shown elsewhere, has been a great factor in Natural Selection, as it must have brought many species of animals to death. Cope (1887) was well aware of this when he called on a " superabundant growth force " to account for overgrowth beyond possible use and often dangerous to the animal in which it acted, though the force of his arguments was much weakened by his unfortunate addiction to undefined psychological terms such as " will." But before any animal gets destroyed by an increasing incapacity to deal with such forced variations, many concomitant changes occur, such as the reactive strengthening of the forehand in the larger more heavily antlered *Cervidæ*. This subject will be referred to later in other connections. It is sufficient here to postulate a viperine dental overgrowth as the main basis of the subsequent co-ordinated changes. If this overgrowth is now advantageous it is clear that the advantage has been gained by a vast number of steps, and that the process of evolution when the organism had to deal with an excessive growth must essentially have been pathological. It seems as if the Colubrines had been able to retain permanent erect fangs only by their being shorter, as is well seen in Elaps. In the Elapidæ the great increase of size has not led to

any heterogonic and vastly increased overgrowth of the fang. It remains relatively short. The early viperine overgrowth must at first obviously have led to difficulty in deglutition. With the Colubrines the act is slow. Elaps is said to chew its prey. This action injects more poison, softens the victim and makes it easier to disengage the rigid fang. In the Viperiforms the fang disengages at once after rapid striking. I have no doubt that this stage has been reached through what must be called forced pathological variations, with all their concomitant and now co-ordinated factors. Can this hypothesis be extended to show, however roughly, that thus the curvature and canaliculation of the fang can be explained?

Dental Stresses.—We know that many snakes, with all their teeth curved inwards, can only with the greatest difficulty, even in the early stages of deglutition, eject the prey if it is anywhere near their capacity to swallow. Thus when two snakes take hold of the same animal and get well engaged the larger snake can swallow, and frequently has been known to swallow, the smaller. This backward curvature even of the smaller teeth is no doubt a " favourable variation." On the lines indicated it cannot be attributed to any accident, karyokinetic or mutational, but to the evolutionary influence of the bending stresses of swallowing. Now the longer the rigid tooth the greater these stresses. The pathological effect of such strains cannot be doubted. On the posterior root surface of the fang there would be crushing, and on the anterior surface, tractional strains. Since we are compelled to infer that the moveable maxillary apparatus is a later development, and since the colubrines without it have a curved, canaliculate, or grooved fang, it follows that even before the viperine development the fang was in a condition to carry venom, and that the poison glands and sac were at least in an early stage of development. Now how and why comes the early groove on the anterior surface? Is it preposterous to

suggest that such a groove is the natural result of a forced partial interruption of the fang's nutritional supply? I do not think so. There would also on the posterior surface be some interruption of supply. The tooth on that side does not grow fast enough in the viperines to maintain its upright form against the recurrent bending stress to which it is subjected. And this bending stress more easily produces its effect as the fang is weakened by the incipient groove. That after many generations, as these pathological states merged into physiological stability, so far as such stability ever exists save in text-books, there was a renewal of growth in the tooth, helps us to see how the groove was so far repaired as to become deeper and finally in so many cases canaliculate. No explanation can, or need, be offered as to why the specific distinctions of fixed or moveable fangs arose. The Colubrine ancestor, as before suggested, may have possessed an initial greater skull stability. But it may be asked why did not the groove disappear by repair or reversion to type? That it did not seems capable of at least the suggestion of an explanation. And here we come, as must be owned, to a curious area in which speculation may seem rash indeed to those who utterly deny the possible part played in evolution by pathological states. For we want to know how the poison sac with its glands originated. Did it come out of the blue, or even out of the ether as lately suggested by an eminent physicist, or as a result of Darwinian accumulated minute variations, not the result of continuous function? I cannot believe it. The teeth, and especially the loosened teeth of many fishes and sea-snakes are poisonous. The more that it is understood that any glandular secretions, even of those of the physiological ductless glands of the mammalian, are poisonous when not met by intra-organic immunization, the easier it will be to infer that the glands about the base of a pathologically strained tooth are likely to be poisonous, and poisonous not only to strange organisms but partially and locally so

even to the organisms producing them. It may then be inferred, without much hesitation, that these pathological excretions, and every secretion is an execretion, served to inhibit dental repair in the incipient canaliculization, in some cases permanently, in others so far as to allow this dental repair to close in the final canal for at least part of its length. Thus and thus only, so far as I am able to see, can we posit a set of antecedents on physico-mechanical lines which roughly accounts for the fang curve and its channel and also for the origin of the poison sac. For if a pure pathologist, ignorant of modern theory in evolution, looked on such an early sac, or even on the later one, how would he describe it and to what would he refer its origin? His natural conclusions are easy to divine. That they would be repugnant to some modern theorists there is no doubt. But then most modern professional evolutionists seem even now totally unaware of what advanced biologically-minded pathologists think of them and their clear-cut theories.

Morbid Origin of Poison Sac.—It is obvious that unless the arguments advanced here and elsewhere, to show that much variation arises in highly disordered conditions, when the organism repairs itself to meet such stresses, are at least considered curiously, it will be impossible to convince the rightfully doubtful critic that the origin of the poison sac must have been essentially morbid. I have, however, shown in other work that all disorder is variation and that every variation must be looked on as disorder itself and the cause of correlated disorder. If that is so there can be no real logical objections to the descent of somatic modifications if such modifications are found in groups and are not lethal, since there is no proof whatever that anything like the hypothetic gene can account for construction. We are finally reduced to protoplasmic potentialities brought about by a complex of internal and external conditions. What is called fixation in the germ is nothing but the

fixed realization of a potentiality, conditioned by the germ's " tools " or catalysts, and if it is acknowledged that injury to the germ can be inherited, as is acknowledged, there is no difficulty in supposing that a morbid variation can and in many cases does, injure the germ. It has been shown elsewhere (Roberts, 1926), that " injury " cannot be closely defined. If I am right in calling, on logical grounds, a somatic disturbance or variation morbid, there is no real obstacle to supposing that this is a regular method of evolution. The germ is no doubt stable and conservative but not so stable and conservative as we have been told, and it is no rash statement to say that evolution is itself a morbid process.

Pathology, Sociology and Evolution.—Certainly pathologists, familiar with the changes, " mutations," and variations which take place in disordered organisms, who observe the common phenomena of tissue changes, such as heteromorphoses, metaplasias, and prosoplasias, will be more ready than most to admit that the potentialities of the whole body must be likewise conditioned by the environment. With John Hunter they must believe that even disease and its results are " natural." They follow on the very nature of reacting tissues, cells and protoplasm itself. If the result finally becomes order, or repair, then that result is physiological. For that is what physiology means. The pathologist may not greatly trouble himself about evolution, but if he does he cannot fail to infer that his own particular study deals with definite factors in the process. It would give him no more shock to hear it stated that the evolution of any particular organ was accompanied in its earlier stages by great disturbances to which the term pathological could be rightfully attached than it would a sociologist or historian to learn that any of our social conditions, now more or less accepted, began in revolutionary conditions and were accompanied by bloodshed. Once more it may be stated that it is in pathology and

sociology that the greatest help to comprehending evolutionary methods can be found. It is almost incomprehensible that their combined study has not been generally recognized as essential for all evolutionists.

Disease and Construction.—If it were not that the whole trend of what has been said, however roughly adumbrated, is in itself somewhat revolutionary as regards current opinion, I should hesitate to assert that the poison sac of snakes, if it is looked on without prejudice, appears for all its differences remarkably like an evolutionary abscess. This does not mean that it is now an abscess. But it is a lined contractile cavity with a discharging duct. This duct is not continuous with the canal of the fang. It ends in a fringed opening which is appressed to the basal part of the fang when the snake bites. This mechanism will remind comparative anatomists of the ciliated fimbriæ of the Fallopian tube and their functions as they clutch the ovary. The sac contains a poisonous glandular product. This secretion or excretion inhibits full dental growth. It is reasonable to suppose that in its earliest form, when free drainage was difficult, the poison, or products of still unspecialized glands, played a morbid rôle in the evolutionary changes of the maxilla and the skull generally. It can, therefore, be suggested that these arose as reactions to the pathological stresses put upon the fang, and that these continued stresses determined in what we call now physiological construction. Some years ago I pointed out that the mammalian heart when cut across and casually inspected, had a most remarkable likeness to a repaired aneurism, and it was therefore inferred that its construction depended, not on genes or mutations of no assignable origin, but upon continuous stresses, followed by hereditary repair, as new functioning called on the potentialities of the sinus and cardiac tissues. Only thus could we explain or understand the extraordinary and very real confusion of the heart mechanism and such changes as those

which had occurred in the prime muscular tissue of the Bundle of His and the supporting mechanism of the *columnæ carneæ*, the *papillares musculi*, and the *chordæ tendiniæ*. It is only by using continuous stresses followed by repair that we can give a reasonable mechanical explanation of the building of the heart. It is a denial of such explanation to attribute the result to any complex of accidents obscurely caused.

The Sac a Sinus.—For similar reasons I utterly refuse to accept the view that even the poison sac and poison glands, which are homologous with the mammalian salivary glands, can be "explained" by spontaneous accumulating variations, or by mutations. To say as much is merely to say that they happened somehow. For we can give no reason whatever for the origin of the variations required, or for the postulated mutations, unless we affirm they were in a gene, or many genes, waiting to be called upon when the time came. Here, in this suspicious sac, is a piece of definite construction to which I prefer to assign certain possible morbid antecedents. We can easily suppose that its beginning was the result of excessive strain and morbid conditions. A hollow human tooth with a discharging root abscess, is a strict early evolutionary analogue. The loosened teeth of fishes which are perpetually falling out, many of such teeth being poisonous, afford another, though less perfect one. Again in the genus Heloderma with its two species *H. horridum* and *H. suspectum*, we find the lower jaw furnished with grooved poisonous teeth. It is the state of the snake's fang with its incomplete growth which affords some of the strongest evidence, or at least suggestion, that this view is correct. If we regard the whole of the phenomena at once, such an hypothesis must be considered. The sac itself, in its earlier stages, was a partly repaired abscess cavity, which by its discharge and drainage through the partly destroyed tooth, did not destroy the animals possessing it. And what can we say of the sac when we find that

in some species, such as Callophis, it extends half the length of the body, though in the cobras it is only one-sixth, while in *Doliphis intestinalis* the poison sac and the glands extend far down the body cavity, "even pushing the heart out of place." (T. Barbour.) An African snake *Causus* has a like elongated sac. The obvious inference is that it is an evolutionary sinus, a sinus forced to extend through many generations by the increased action of the glands concerned. That all this machinery arose from a minute advantage is absurd to suppose. That it sprang from an original large mutation is even more ridiculous. Loath as I am to label any process such as this with a name, since to none can complete credit be given, it seems only right to say that a more modern Lamarckism than that often advocated seems to give us the real germ of explanation. It is obvious that in a lethal and limiting sense Natural Selection has here played a great rôle. It is impossible to imagine that ophidian evolution could have taken place without the survival of the fittest. And when we understand that the fittest are those in which the power of reactive repair achieves a new if temporary stability, with what we call physiological or reasonably static working results, the phrase really means something and ceases to be a mere logical *circulus*.

Analogous Poison Glands.—Possibly something may be added here as regards other poisons employed by other animals than snakes which seem to strengthen the case put forward. In every case there is a gland or several glands concerned and in some the apparatus employed to excrete the glandular products seems remarkably like the later organization, with changed use, of a primitive nephridium. The functions of the ductless glands, every day becoming better known, are now directed to metabolism, maintenance and construction. The body has "learnt" or is learning, to use their products. For evolution has not ceased. They must once have been excreting glands.

May we not say that if any excreting gland with free external drainage could be permanently obstructed without death ensuing, construction would be altered? But continued free drainage of a poisonous product must also alter local construction. The action of the excretion must be met by repair and defence of the tissues through which it passes. So we get the sting-rays and weaver-fish. The defensive armament of the mephitic skunk, in which the overgrowth of certain rectal glands, homologous with less offensive rectal glands of all the carnivora, has altered habit and therefore probably construction. But one of the most remarkable analogies with the ophidian apparatus is to be found in the Australian platypus (*Platypus anatinus*). The suggestion that the poison sac of snakes is a " physiologized " sinus begins to seem more than a guess, and a wild guess at that, when we see that the Platypus has on its hind heel a strong, curved, and moveable spur, attached to the tarsal accessory bone, which carries a product poisonous enough to set up much local disturbance in those wounded by it. How did its poison arise? Not locally, for it is conveyed from a large gland on the back of the thigh by a minute duct, opening in a fine slit on the spur. If this duct cannot be legitimately described as an evolutionary sinus I do not know how to name it. We need an exhaustive monograph, such as I have been unable to find, on the whole range of animal poisons.

Armed Poisonous Fish.—The sting-ray, such as the *Ætobatis marinari*, is devoid of specialized poison glands or of any canal in its spines. It has been suggested, however, that the mucus which is secreted on the surface of the fish is itself poisonous and is conveyed into wounds by the jagged serrated weapons of the tail. In the *Trachinus*, or weaver, the dorsal opercular spines are deeply grooved. *Synanceia verrucosa* has a more elaborate apparatus. The dorsal spines in their terminal halves have deep grooves and poison sacs. These sacs are prolonged and lie in grooves

and are open at the point of the spine. But among fishes the most remarkable analogy with the snakes is seen in *Thalassaphryne*, a Batrachoid fish from Central American coasts. The operculum, which is very mobile, has a spine which is canaliculate and open at its base and point. A sac at the base contains the poisonous fluid. But here, too, there seem to be no specialized poison glands in the sac. It seems impossible to attribute all these weapons to anything but a complex, slowly co-ordinated process of forced pathological changes, gradually stabilized until they became what we call physiological conditions. The same may be said of the scorpions, in which there is a poison sac and sting with a duct attached to the final fifth segment of the tail. To conceive this kind of machine originating in a small useful variation or even in an early stage as a definite mutant ready to function, is too great a strain on the imagination. The conception that accidental disorders, dependent on all such factors in the environment as bring about pathological conditions, may easily be partially repaired and finally used, puts no such stress on those who recognize that pathological conditions which are not actually destructive, are a great factor in individual development, and who also observe that in the phenomena of gestation, parturition and adolescence we are still confronted with evolutionary and evolving processes. There is, however, no need to labour these points for any student, not blinded by teaching and text-books, can work them out for himself. It should not take him long to determine that physiology is an abstract science, that the "norm" has no more existence than the "average," and that much to which he is accustomed to look upon as settled by evolution becomes on close inspection a patchwork of disorder functioning poorly and determining further change. All the examples quoted above strengthen the view that unavoidable forced changes can be "made use of," as Darwin himself acknowledged of variation generally, though he

laid no stress upon the suggestion and certainly missed much of its wide application to the whole subject of adaptation. It seems that we may say that much construction in one sense is accidental. A new line of construction may arise, not as a mere addition to what is established, but as the result of some unlooked-for natural result of glandular overgrowth, the causes of which may be highly obscure, though probably traceable in most cases to the environment, or to intra-organic immunization. Assuredly no pathologist, with any love of biology sufficient to make him lift his eyes from the microscope, will think such suggestion as in any way fanciful. Perhaps some might easily agree that many common disorders of human function in what we call civilization are but signs of further evolution in a rapidly changing environment.

"*Idols of the Den.*"—That such suggestions will be received with favour or even with patience by those who are satisfied by the progress of explanation on accepted lines, would be too much to hope. They afford materials for nothing but a very rough sketch of a most complex process evolving during a long period of time. But at least they offer, as it were, a physico-mechanical diagram, for the definite changes are given more or less definite antecedents, which explain, or at least give reasons for, evolution on a certain line. This can be afforded only by taking into consideration breakdown and repair and the very obvious capacity of protoplasm to build under a changing direction of energy new and changing structure, so that the various tissues and cell colonies can carry on their old work until that altered structure is functioning on new lines. The doctrine of genes merely assumes that characteristic construction can be carried out "as arranged." This is not real epigenesis, but pseudo-epigenesis and it bears signs of a suspicious descent from the archetypes of Cuvier, modified no doubt by Weismann. We learn nothing of

processes by such assumptions, and it is with processes and the linking up of antecedents in such a way that co-ordination appears naturally, that science is concerned. This is recognized by most scientific workers but not by all orthodox biologists. A chemist who employed their methods for investigation would gain little credit. We have reached a strange pass when the very prophets of experiments fill up huge gaps with ghostly hypotheses and treat them as realities. It is, perhaps, not merely haste which leads them to do so but certain semi-religious "idols of the den" of which we have had lately strange evidence.

Injury to the Germ.—There is, however, nothing mystical in the view that forced somatic modifications in groups under similar stresses can induce, by "injury" to the established physiological action of such groups, bio-chemical changes in ovum and sperm, which again compel changes of energy direction and growth *in embryo*. For such a statement implies taking into consideration all that we know of glandular influence and immune action, and naturally includes known pathological phenomena and function as determining not merely the construction of the soma but the bio-chemical armament of the germ. The argument against this view which is held to have most force, that it lacks experimental proof, utterly fails in the presence of natural experiment as seen fully developed in actual bodily construction *in embryo* and in the anatomy of the adult and also in the vast laboratory of natural experiment of disorder and disease. It is scarcely too much to say that every disorder known to physicians and pathologists demonstrates the native and natural power of protoplasm to adjust itself until it is over-whelmed and destroyed. That its more normal changes during stress under environmental conditions should affect the germ, by "injury" if we like to put it so, is just what might be expected. This is really the only assumption

needed, and every day evidence accumulates to show that the bio-chemical "skeleton" of any organism is marvellously flexible, however stable it may seem in what we call "normal" conditions, which as a fact never exist and never can exist. For that one very likely assumption we are asked to substitute chance and unexplained genes and mutants and mutations, which taken all together leave evolution in the air. It is scarcely a wonder that such views have practically invited the late flood of obscurantism poured over us by religious physicists.

The Time Factor.—Nothing has been said here of the time factor. We hear of experiments done to prove Lamarck was right. Something has been achieved. But nothing convincing. Lamarck himself would be the last to think that such experiments would bear fruit. He was amazingly strong on the length of time required for the evolution of changes in organisms. When treating of species he says that the principles set forth show "That all the organized bodies of our earth are true productions of Nature, wrought successively through long periods of time." In the section "General Principles" he speaks of the enormous time required to bring animals to the complexity we now see. And later he adds, "There is one strong reason which prevents us from recognizing the successive changes by which animals have been diversified and been brought to the condition in which we observe them: it is this, that we can never witness these changes." Prejudiced critics of his theory have ignored all this. They have preferred to fix their attention on his incautious use of the word "all" when he speaks of all the acquisitions or losses wrought by nature in individuals as being preserved by reproduction. Assuredly he did not mean "visibly" or "at once." What he said of the time factor again and again must be taken as considered comments on his general rules. I shall refer to this part of the subject when dealing more particularly with his Principles.

Nature of Mutations.—I am ready to admit it must be convenient to have at hand a mutation system which can at once supply variations fitting established structure, even though many fail. But variations ready in the germ, which deny any causal connection between the established structure and the environmental functions, are truly a denial of any physico-mechanical explanation, and so far no one has shown that mutations themselves are more than intra-specific unfixed characteristics. If they are ever demonstrated to be generic something may then be said for them. Those who like to see at least a suggestion of the definite linkage of antecedent states and infer from somatic phenomena in health and disease that changes, except in unimportant variation, occur only as reactions of the protoplasm in definite cells and tissues, can therefore take little interest in them save as a passing phase in the history of biology. I agree with one eminent and somewhat cynical authority, with whom I discussed mutations and genes, that in ten years we shall hear little of them. They will in their time, after a fluttering and ephemeral existence, go where Hadrian and Augustus and Weismann have already gone.

If in this long and perhaps wearisome journey from Exmoor I have ventured, as some may believe, into realms forbidden to sober speculation, I would ask those who think so to consider whether they are not far less remote and mystical than some in which many of our most respected professors move with priestly confidence. The methods of evolution suggested are certainly not mystical and the thinking involved is crude and brutal realism rather than fanciful. It is vain, however, to suppose that the rough scheme of change and co-ordination suggested will at once, or soon, find supporters. It is almost as easy to destroy the ancient superstitions of religion itself as it is to eradicate those accepted in some theories dear to scientific orthodoxy. It may be a wrong surmise but many reasons can be brought

for believing that their persistence is due to their connection with latent religious sentiment. Thus there is preserved a place in the scheme for something supra-mundane which forbids the acceptance of the physico-mechanical or physico-functional explanation.

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CHAPTER TWO

Vespiform Moths and Mimicry

A Difficult Case—Pseudosphex—Useful Initial Variation—Segmental Construction—Apparent Pedunculation—Pigment—A Hard Case of Mimicry—Birds and Butterflies—Energy and the Lepidoptera—Catalysts, Pigment and Immunity—Larval Nutrition—True and False Mimicry—*Emus Hirtus.*

A Difficult Case.—Individually we owe a great deal to chance or accident, for a very small variation in our environment may lead to other functional variations and thus our personal heredity may develop all kinds of desires, needs (*besoins*, shall we say, of Lamarck?), and in the end much may come out of it, or little, just as fate brings it about. Thus if Exmoor and an engineer led to the beginning of this book I may put this chapter down as due to a well-known field Lepidopterist, whose name distinguishes very many butterflies and moths of his own discovery, whom I met in Costa Rica. For when we discussed in San José some of the questions dear to all Lepidopterists, such as mimicry, we came at last to vespiform moths of the genus Pseudosphex. Now what I know of the *Heterocera* is as little as may be, but when my new acquaintance suggested that the most complete case in all mimicry was the instance of the moths which took on a close approximation to the general appearance of wasps, we naturally went on to discuss the great recurring difficulty met by pure Darwinians, that is by those who have not joined the semi-religious sect of the Positive Genetic Mendelians, when it is asked how an

original minute variation can afford ground for Natural Selection to work on. For, here again, in this case of Vespiforms, natural selection means very simply the enemies, often the very wasps mimicked, of the minutely changed organism. The Mendelians, who can produce anything but final conviction in Neo-Mendelian truth from their new Pandora's box, avoid this impasse by giving us gladly just as big a variation as may be needed. Of this I may speak later when discussing some other aspects of mimicry in which even accidental minute changes may be of use. As the discussion continued it became necessary to develop very shortly the rôle played in evolution by pathological states, by breakdown and repair, by such conditions as arose when morbid conditions were physiologically healed. For in this way, as previously shown elsewhere in detail, we can imagine forced large definitely caused variations arising on which Natural Selection can really work. What follows here is indeed developed from this casual interesting talk with a field naturalist who, like nearly all his kind, is not rabidly hostile to something resembling the views of Lamarck.

Pseudophex.—That a harmless moth can take on a likeness to the armed and predatory wasp is certainly most remarkable. For the likeness apparently not only deceives the armed female wasps which prey upon them and their larvæ, but may in many cases also deceive any but the highly instructed human observer. And during the whole of their mature lives they seem to live in continuous contact with the true wasps. They haunt, as I understand, not merely the same locality but the same species of plants in which the wasps make their nests. And there they lay their eggs. If this is so we are in some difficulty when trying to explain why their instincts of self-preservation did not lead them to choose other places. Doubtless during long ages colonies have split off from them and may have founded other species. But some must have

clung to ancient ways through an overmastering instinct, attaching them to definite vegetation necessary for their larvæ, which forbade migration. This probably indicates, even if the mimetic changes did not do so, the lapse of an immense period of time. No doubt the longer the postulated period the better for the case of the pure Darwinian but, long or short, we are still in the dark as to the initial change which helped the early ancestor of *Trichura dixanthia* (Hampson) of Brazil, or *Syntrichura virens* (Butler) of Guatemala, to survive. I have chosen to mention these two moths for reasons which will presently appear. It must be suggested that the use of a common habitat by such moths and the mimicked wasps commenced at the period when the moth was not yet properly a moth but an early lepidopterous form and when the wasp itself was yet highly primitive. Such a hypothesis not only gives evolution time but also affords grounds for supposing that some early condition of the environment may have led to the pedunculate form of both wasp and moth, and also have led, still more remarkably perhaps, to the very significant changes in some moths which are not pedunculate but merely suggest such a shape. To what extent they have other dangerous enemies in the imago form is not clear, but the chief are always wasps themselves, as the mimics, all of the family Syntomidæ, which are vespiform, are day-flying. So far as I am aware no night-flying moth is ever vespiform. What then is the possible machinery of this curious transformation? It is a machinery which we want, however crude, not pure imagination. Absurd as it may seem to the devoutly scientific, for in them there is much faith, especially where mimicry is concerned, it is my purpose here to show roughly a series of macroscopic steps which might satisfy one accustomed to build a house or a machine rather than a zoologist who believes he is thinking when he is simply accepting Bates or Muller or such a faithful disciple of Mendel as Dr. Punnett. We

can have too much faith and too much authority. So I leave Mendelism alone and here discard the early minute variation. For who but the enthusiast can believe that casual undirected, and possibly contradictory and nullifying, variations brought about this miracle of mimicry, by chance karyokinetic changes, without any variations of somatic origin being preserved by hormonic transmission? Again, what would a mathematician say of the primitive moth's chances of becoming a decent copy of one of the Diploptera? Here, too, it cannot be said that Lamarck can soon, or at once, give any help beyond his working assumption that somatic changes could be inherited. No Neo-Lamarckian goes as far as his Master's 4th Principle: "All which has been acquired, laid down, or changed in the organization of individuals in the course of their lives is conserved by generation and transmitted to the new individuals which proceed from those who have undergone these changes." For this, it can readily be admitted, is a gross overstatement which omits the strength of reversion to type due to ancient endocrine and hormonic influences not easy to deflect.

Useful Initial Variation.—But suppose we put aside orthodox and unorthodox alike and imagine that some particular somatic change occurred, not a minute or casual or karyokinetic change but one large enough to make some of the moth's enemies mistake it for a wasp, and then even go further and show reasons for this being inherited. When once a change of this order has occurred we may allow Natural Selection its proper place, for we can admit, given something visible and appreciable by the living agents, or the natural selectors, that then every casual or consequent variation on the same line should tend to be inherited. Now what change of form, what deformation in fact, could occur as a disorder, and a very possible disorder of growth, which might at once suggest to the altered moth's enemies that it was not a moth at all but that it had, on a casual glance, a resemblance to quite another kind of insect?

I suggest that a failure in segmental growth, due to disease or a local growth factor, might so constrict the moth as to give its upper abdominal segments the typical pedunculate or stalked look of the *Hymenoptera aculeata* and thus afford real ground for Natural Selection to work upon.

Segmental Constriction.—It is certain that such an explanation, especially when further details are not worked out, must almost inevitably seem absurd to the ordinary students of mimicry. They have not been taught to consider abnormal changes, or malformations, as possibly heritable. The results due to ancient conceptions of perfect archetypes still dominate them. But may we not ask how far they themselves get in explanation? Can the ordinary incredulous inquirer meet the drafts they make upon faith? The truth seems to be that here the common Darwinian explanation of mimetic resemblance falls to the ground. And yet can the one offered take its place? Not, it must be owned, without more evidence, or at least suggestion, than that yet brought forward. For here we have got no further than a moth with a constricted abdominal segment. This no doubt gives us a moth with a casual resemblance to the wasp. But are we in any way justified in assuming that such an hormonic accident can happen? It seems that the obvious answer is that it happened to the early wasp. Yet in that case, as in the case of the moth, how did it come about? Possibly, even probably, by a failure of the second abdominal segment to grow while the others increased in size. But again, how did the failure take place? We do not know, even though it may be suggested that it came about in both early forms from similar, possibly nutritional, causes, but if transmission of somatic changes does take place, we may guess that this was a morbid failure, to some extent made use of later by giving the insect greater elasticity of movement than if it had the whole segmental armour. We may infer that what happened to the wasp happened afterwards

to the moth. To the pathologist there is nothing very remarkable in such a growth disorder, however rash it may seem at first to suggest that growth disorders may end in being specific characters. But why not, if they are failures in hormonic construction, and if we assume, as we must, that hormones, or at least enzymes, catalysts and electrolytes, are in fact part of the hereditary chromosomatic tools of construction? But is there any other evidence for the particular suggestion made, that is, that disease or disorder caused this favourable and appreciably large inherited variation? There is some remarkable evidence which may lead us much further than a simple failure in hormones, however that came about, and might suggest that a local disease or infection, contingent perhaps on a local low segmental metabolic rate, can in certain cases, where many individuals become affected similarly, lead to inherited constructional differences, as it may lead to tumour construction in single cases. Let it be owned that any such theory or hypothesis does away with the simplicity of some other popular explanations, but that can scarcely be helped if such clearly fail.

Apparent Pedunculation.—The further suggestive evidence to which I have referred is found among those of the *Heterocera* which are not truly vespiform as regards pedunculation but are only partially so, or possess characters suggesting that form without its being present. There are also mixed cases in which partial or slight pedunculation is reinforced by these other characteristics. Some of these characters are found in the moths previously named, *S. dixanthia* and *S. virens* and also in *Corematura aliaria* (Druce). *C. aliaria* has the abdomen blue, but the first and second segments are white. There are like marks in *S. dixanthia*, and *S. virens* is slightly pedunculate with adjuvant white patches which distinctly aid the vespiform appearance. In a South American moth, *Argyroeides braco* (H. S.), the whole of the third abdominal segment is

white as well as somewhat pedunculate. In *Argyroeides eurypon* (Druce) the first segment is wholly white. In some other moths which are not vespiform this curious local failure in pigmentation is also found. In them the marks are very large and variable. Now it is certainly remarkable that such a failure should assist in making the moth look like a wasp, as it obviously does. I know not what others will think, but to me it is suggestive that here, and in the other cases of a common failure of segmental expansion, is the early rough actual or apparent change on which Natural Selection could work. And this means that the wasps, especially the early ancestral wasps, and birds as well, since birds do actually attack moths, even if they usually ignore butterflies, saw in it something which might suggest to them that the moth was not a moth at all. But can anyone with a free mind, one not hampered by prejudice, believe that this local whiteness, so strangely shaped and placed as to conceal, or at least partly to conceal, the moth's actual abdominal form, began on both sides as accidental minute variations? And could a single minute spot on one side afford the first moth in which it appeared any protection? It is absurd to say so. And, if two spots appeared, are we forced to infer that they appeared simultaneously by accident, not from some internal or external environmental causes but from some very remarkable linked double germinal change? I do not see how anyone with a logical endowment can arrive at such conclusions by any set of syllogisms of probability. Surely no observer, who had never been taught that changes wrought by the action of the environment are tabooed as factors in inheritance, would come to such conclusions. Even if an unlikely coincidence in germinal change took place and was miraculously of survival value owing, we must suppose, to the terror inspired in all the moth's enemies, including wasps, by two hitherto unobserved small white spots, are we to believe that further germinal accidents worked out, by

the accidental addition of other minute white changes in pigmentation, the curious effect which we now observe and which may truly be of value to the insect? No a priori conclusions compel us to accept a view inherently unlikely, when we know that in innumerable instances changes in pigmentation, local or general, do occur owing to peculiar disorders. There is much medical work done on pigmentary plus and minus changes. I come to the conclusion here that some such disorder, due to a local infection or to some still unrecognizable failure in a colour enzyme, did actually and at once afford the first protection that all our general views of mimicry require, and this case alone is sufficient to throw the very gravest doubts on the orthodox theory. When the case of all these moths and many others is considered in this way we are forced to the conclusion that somatic changes due to the environment must in many cases be inherited. And when to these materials for such an argument we add those which I have previously drawn from a study of general mammalian anatomy the evidence seems overwhelmingly on the side of transmission. It may be added that the observer in Costa Rica, who begot this particular discussion, brought up these later cases as an argument against my views of the morbid causation of constricted segmental construction, but in the end owned freely that they did, in fact, seem to strengthen them. And here, as a note, I may add that an example of changes in a young grasshopper of East Africa, *Eurycorpha*, exactly like those in the vespiform moths, is given by Haldane and Huxley (1927). These workers are extremely unlikely to agree with any conclusions of my own, and if I understand them they attribute the young grasshopper's pale segmental patches and aculeate look to pure Natural Selection. Faith it seems can remove mountains of difficulties.

But does the view put forward get rid of all our difficulties? Assuredly it does not. We shall have to

account for all the subsequent changes of the moth towards the wasp form. But provided they can at last swallow occasional transmission the Neo-Darwinians and Mendelians should be the last to boggle over these. If the whole transformation could, as they think, come about without any reasonably obvious point for Natural Selection to work on, they cannot object to the task being finished by that agency when there really is something given of obvious survival value. Some of the moth's subsequent changes may have been correlated with the abdominal constriction and have come about directly as they must have done with the wasp itself, whether that took on its form because of a similar accident, or whether, as seems far more likely, the peduncle represents the normal size of the primitive abdomen and the later abdomen a great enlargement of the primitive size.

Pigment.—Though I am convinced that the forms with the white markings must have arisen in the way suggested, the whole subject of pigmentation, or its absence, in symmetrical positions on the bodies and wings of animals and insects is one of immense difficulty the moment we face it. It is true that symmetry itself in bodily form seems a *sine qua non* in most animals, and we can form theories of its origin without difficulty as mere cell division seems to be its natural basis, but why does colour follow laws of symmetry in so many cases? We can, I think, in our present state of knowledge say no more than that venation and nerve tracts, which must be more or less symmetrical, dominate colouration. And we know, just as we know venation and nerve tracts are never exactly symmetrical, that colour is also variable from insect to insect and even from wing to wing. These colour characteristics are probably not due as a rule to any possible survival value, they are forced environmental changes, though when they arise they may indirectly tend to dominate the habits of their possessor. They are then a determining part of

the environment and I find very little difficulty in believing a bird or insect is far more " conscious " of itself than most experts in human psychology seem able to credit. The question of the abundance of colour in the Lepidoptera which makes them so conspicuous cannot be divorced from other questions of the same kind in birds, and in both cases I prefer the opinions of Wallace and Hudson that excess of colour and general excess of ornament are due to excess of spare energy in the animal, and its absence to lack of energy, scarcity of food, and absence of photic stimulation. Both colour and ornament may, indeed, be handicaps and as such they help to determine habit and the locality affected by the species. In any case we may say that no simple dogmatic explanation will greatly assist us to discover the mechanism of colour and form, whether mimetic or not. For in many cases, as might be guessed by pathologists, it seems to depend on forced excretion of katabolic products not carried off by anything like nephridia.

A Hard Case of Mimicry.—Some Lepidopterists often say that the vespiform moths present the highest form of mimicry. In what way are its causes more remarkable than the causes which led to a butterfly being practically indistinguishable from a leaf with a leaf stalk ? It is true that the sound Darwinian with a firm belief in the miraculous powers of Natural Selection may find no difficulty here, and if I say that in a case of this kind I am at times almost compelled to agree with him, this may be taken by the charitable as some compensation for general heterodoxy. In this case it seems reasonable to suppose that those early butterflies which selected as points of rest, and possibly breeding, trees with leaves of their own colour thereby gave Natural Selection something obvious to do. But if there was no other factor leading to actual leaf form than the postulated undirected Darwinian variation it is scarcely a wonder that a kind of orthogenesis still flourishes among

us. For we may again ask what a student of mathematical probability would say as to the likelihood of the result we see being attained in anything under an infinite time. And now it must be owned that if choice has to be made between what may seem to many of us absurdities, I am here almost inclined to fall back on the purest Lamarckism, provided I am permitted to interpret Lamarck's " need," or desire, which is a form of *horme*, in my own way. It is absurd to interpret anything instinctive as a purely mechanical act which does not take into account the nervous system. I am therefore led to infer that any instinctive act, when successful, is probably in the meanest insect accompanied by those motor-changes which we term emotion, and that a failure, such as often occurs, is accompanied by emotional disturbances which we term in the lump " disappointment." It may, therefore, be suggested that within the limits of a bird's or insect's life activities it is as truly conscious as we are within the limits of our own life activities, for no human prejudice or religious prepossession need lead us to the absurd position that only the biggest brained animals can possess those complex reflex activities which we call for short " consciousness." The belief, founded not obscurely on religious prepossession and human " free-will," that there is something radically different between human " conduct " and animal behaviour is somewhat extravagantly illustrated by the following passage: " Animals are often kind parents, affectionate lovers, helpful to their kindred, devoted to their society, but there seems no warrant for supposing they ever think of their duty! They may be good and kind but they do not know what ' ought ' means." (J. A. Thomson, 1929.) It might be imagined that I have here illegitimately put in the note of exclamation. This is not so. It is indeed highly remarkable, when evolution has so fixed its social conduct and behaviour that the beautifully natured bird or animal can do no other than be kind and helpful, that

it should be less regarded than the human being who is too often far less perfect. The argument is as baseless as those of some moralists who consider that a man who wants to do evil and after a struggle avoids it is better than he who has no evil in him. To do one's duty, should one have to think about it? Would such a marvel as a deontological duck pondering on "ought" be a better duck for that? This may be only by the way, but, if we admit, merely for argument, that animals, and even insects, know enough "to go in out of the wet," if such a picturesque colloquialism is permitted in science, and are aware of what they do and how they do it, and very often why they do it, how far can we go on such a supposition with regard to the leaf-like butterfly? It may be moving on an unstable bridge across very deep waters if it is said that there seems no insuperable difficulty in believing that the incomplete butterfly was so far conscious of its environment as to perceive a dim likeness in itself to a leaf. Is this so absurd? Leaves and flowers make up a large part of the insect's life. It recognizes those it feeds on, those in which it must lay its eggs. It recognizes its fellows and its enemies. Where then is the radical absurdity in supposing that it also recognizes where it is safest and why? It may be added that those who follow the advantageous variation view, which implies that a very small variation can be recognized at once by enemies, should be the last to deny that insect and bird brains are capable of rapid judgments on such a minute variation even if they are wrong. This negatives the common view of instinct as "unconscious" automatism. Can we not guess that at times this butterfly mistook a leaf for one of its own species, and if so is it sheer madness to imagine that it might in the cells of its small but obviously acute and rapid brain, even "wish" that it was like a leaf? But if so what then? Are men of science to be asked to believe that for a butterfly to want to be like a leaf is sufficient to make it like one? By no

means, but we may infer that the " wish," which is a physical and molecular molimen, had some effect on morphallaxis during thousands of generations, largely spent in this particular environment, though no one has yet fathomed all the machinery by which the nervous system can divert energy and turn it into new structure. And while leaving this problem, which I shall return to at a later chapter, thus unsolved, we may reflect that the belief that the external environment can at times have a very definite effect on growth, and even colour, through the nervous system, is very old indeed and may not be wholly without foundation. We may infer it from the colour changes of chameleons, in which such a change might easily get fixed in a stable environment of one dominant colour.

Birds and Butterflies.—But before leaving the vespiform moths it can be said that pure imitation of one insect's habits by another may have actually led, not only to a greater likeness in such habits, but indirectly during long generations to changes in neuration and structure. Observation of vespiform moths of the genus *Pseudophex* shows that some move the abdomen as wasps do when threatening to sting. Other like motions have been noted, which suggest imitation. We have not yet learnt what our own complex nervous systems can do, and I venture to think that few biologists have yet come near imagining what a mere speck of brain is capable of achieving in any insect. After dealing so lightly with vespiform moths, some consideration of another aspect of mimicry may not be out of place. So far little has been said of Mendelian mutations and the place claimed for them in protective resemblances among butterflies. After some slight experience in countries where insects of all kinds abound I believe it may be said that systematists seemed to overrate certain factors in the problem. It is true that some who were not systematists but great observers, such as Wallace, Bates and Belt, had given everyone to understand that the chief

agents in the elimination of the more luckless species and variants among butterflies were birds. This I have doubted for a long time and have, indeed, come to believe that of all its enemies from the egg to the perfect imago, the butterfly has to fear the bird less than any. Without being a professional or professed observer one may at least be commonly observant, and having had opportunities, denied to most systematists, of experience in Australia, Western America and Canada, Nicaragua, Costa Rica and other Central American republics, to say nothing of other parts of the world, I can say that only once have I seen a butterfly caught and killed by a bird. When above Peralta in Costa Rica where butterflies and birds abound I took special note of the absolute immunity of the *Lepidoptera* as far as their supposed avian enemies were concerned. Not once in weeks did I see any sign of a postulated elimination of edible varieties. If it is said that this was due to the fact that the Costa Rica butterfly has reached so complete a state of perfection that selection ceases to operate, a possible but highly improbable solution if indeed birds fed on butterflies at all, it can scarcely be argued that this is the case in all the other countries mentioned. I made the same observation in North-West Nicaragua. On reaching the conclusion that butterflies were practically, if not entirely, immune from avian attack, it seemed to me that this contradiction of high authority required corroboration. Since then I have questioned every observer I have happened to meet, and though some told me that they had on rare occasions seen birds kill butterflies, many owned, though they had always taken the fact for granted, that they had no experience to support the general belief. E. H. Aitken, who wrote some charming books on animals in India, says: "Butterflies enjoy a strange immunity from being eaten . . . birds leave them alone."

Wallace speaks of occasional patches of nipped off wings lying on the ground in tropical forests. But were these

rejected wings rejected by birds? Judging from what I have seen, as well as from what I have so rarely seen, it seems far more likely that the butterflies to which the wings had belonged were caught by lizards or monkeys concealed among the branches of trees rather than by birds. It has to be remembered that the abundance of food in the tropics renders it highly unlikely that birds should waste their time in chasing butterflies. Clearly the greatest enemies of all the Lepidoptera are those who eat the eggs, the larvæ and the pupæ of such insects. These are the prey of all lizards, and even the small monkeys, as well as the numberless army of their insect foes, such as the stick insect, the mantis, and a thousand others. It may therefore be suggested that all the theories founded on the existence of what may be called generally *Alpha edibilis* and *Alpha inedibilis* are, so far as birds are concerned, without a sound foundation. So long as a butterfly is flying it seems immune. If it were really exposed to being caught on the wing we should expect it at the least to show alarm when birds appear. A bird should have on it the same effect that a hawk has upon small birds themselves. Those who have seen a dozen species of butterflies over a flowering tree with birds about them, of which they take no notice, while their supposed enemies utterly ignore them, must surely doubt the current theory. Some views held by certain picturesque naturalists on Nature's fertility in schemes to carry on the butterfly tribe are almost amazing to an unenthusiastic observer who has reason to doubt the popular theory of survival value. Every mark, bar, spot or stripe, every overgrowth, every arrangement of colour is held to have its particular use. The result is that birds are exhibited to us as a race of semi-idiotic animals which grasp at a spot upon the edge of a butterfly's wing and therefore miss the butterfly, which gets off at a very small sacrifice. The same is said of the swallow-tailed butterfly. This overgrowth has

"survival value," or so we are told. But we rarely hear from these naturalists anything of the overgrowth or conspicuous colours which are obvious handicaps. These are frequently wrenched and tortured into some kind of use, or the writer remarks hopefully that their use has not yet been discovered. I may have something to say of this later when dealing more particularly with protective coloration, a subject marvellously fertile in fantastic theory.

Energy and the Lepidoptera.—If what has been said approximates to correctness it seems clear that the various forms of so-called mimetic resemblances among the day-flying conspicuous *Lepidoptera* need further explanation. Is there any other, even a mere suggestion, close at hand? Possibly there is if we consider the nature of colour and ornament and the functions of superfluous or free energy. Is it not curious to observe that the *Rhopalocera*, or butterflies, and the *Trochilidæ*, or humming-birds, have certain qualities or characteristics in common? They both have a peculiar swiftness and elusiveness in flight, they both exhibit an amazing variety in colour, whether pigmentary or interference colour, and both seem when flying to be beyond and above all danger. If Wallace and Hudson were right in supposing that all birds with the males especially bright in colour and adornment owed their endowment, not to sexual selection, but to their possession of excess energy bourgeoning into colour and remarkable feather structure, may we not say something the same of butterflies? This view of these two great naturalists has quite lately been reinforced and practically proved by Eliot Howard (1929), although he does not deal directly with such particular energy overflows. We may know nothing of energy except that it does work. But when it runs down from potential into kinetic energy, we see work done and structure built. Colour and pattern in superfluous structure are work done by the energy that birds and butterflies have over and above their bodily needs and activities. To grasp this does not

seem easy for everyone. Let us take a concrete human example, the building of a house. It is obvious that the available energy which can be put into its construction is measurable by money. If a man sets aside a thousand pounds and builds a house which takes it all he will not be able to decorate it or make a garden. But supposing that he has the thousand pounds and spends no more than three quarters of it on the house itself, he then has available, when it is built, a quarter of the money for decorative purposes. Now a bird or butterfly has so much money to devote to building itself and to live on, and we infer, on the general physical grounds that work cannot be done without the expenditure of energy, that it is spare or surplus energy which gets into patterns, colour, and ornament of every kind. The butterflies and the hummingbirds are in a sense above the common factors of natural selection and they are both of a prodigious energy. In such cases we may expect the greatest variation. Their natural immunity puts them into the position of domesticated and sheltered species of animals which, as Darwin knew, become highly variable. They are not protected because they are variable and can thus afford natural selection opportunities. They are variable because they are protected. How often do we find that the best way to solve a problem is to invert it ?

Catalysts, Pigment and Immunity.—How comes it then that this increased variability leads to likeness among different species ? We know little enough of animal colour and its origins. And yet we do know something. *Pieris* is known to use urea and tryptophane to build up pigments. Chocolate-coloured rabbits use special enzymes and probably tryptophane as well. Botanists will remember that plant alkaloids, whether they influence colour or not, seem to be katabolic and excretory in nature. Pattern follows neurational and venational tracts. But how and when and where do insects pick up or acquire the factors which

determine colour? A long search among Mendelian authorities has thrown no light on the question. We learn from many that though factors can be lost they cannot be gained, or acquired. This seems, or once seemed, the proper Mendelian view. But as a matter of fact we know that a factor such as iron, necessary to red-blooded animals, is picked up from the environment. Once it was acquired, that is, was picked up and used, it determined metabolic rates if nothing else. Goodrich, following O. Herwig, says that the acquisition of factors is the question now before biologists. This almost seems a concession, for it does at least contradict the practice among Mendelians of so ignoring the factors as to suggest irresistibly that they were all packed up in the primal germ. It is more reasonable to suppose that a changed environment brought in new agents, new factors, and that these came in through the processes of nutrition, for we know as a fact that foods can change bird coloration. Elsewhere I have shown reason for believing that nutrition is a process of immunization, that digestion is breaking up alien and harmful products and immunizing them. But in the evolution of these processes many animals were poisoned and destroyed. Physiologists and physicians recognize the fact that not every common food protein can even now be used by all. Asthma bears witness to this. We know the effects of some foreign proteins, but once most proteins were poisonous or at least disturbing. The animal body was conditioned from the first by the necessity of finding ways to use them. Physiological anatomists are aware that the very form of the intestinal tract is determined in its characteristics not merely by normal functioning but by the functions forced upon it by excessive or uncommon food. So food is a "factor" determining construction, and most anatomists and palæontologists will find no need for the explanation of such constructions. Even now among human beings food and digestive disorders are determinants of structure and

function. It is clear that the general state of health in groups of organisms such as human societies is a perpetually working factor of constructive, not merely lethal, evolutionary processes.

Larval Nutrition.—And so we may return to the *Lepidoptera* and try to learn in what way such seemingly irrelevant details may help us to explain false mimicry. It is known that butterflies usually lay their eggs in special selected plants. Their acquired reflexes force them to do so. Pavlov has helped us to see that instinct must be analysable into reflexes and has thus made a notable contribution to biology and general zoology. For, so far, I have not learnt that there are genes for instincts. Any instinct is a complex of reflexes which must have been transmitted. Is it making undue use of speculation to say that a prolonged era during which butterfly larvæ were specially fed has helped to determine their characters and colours? We know how special food can create a queen bee from a neuter larva. It is, at least, a reasonable suggestion that it is during the larval feeding stage that any new acquisition can be made and worked out, perhaps destructively in millions of cases, during larval and pupal development. Butterflies migrate, if not willingly, at least when forced by the wind and the seasons. A variety related to the normal *Rhopalocera* of a given area may then be forced into that area. It may not find the exact plant used in its former home. It may find another variety which serves its purpose. If the coloration of the normal inhabitants is due to historic larval nutrition, there is nothing easier than to suppose that the immigrants in a few generations, or even suddenly in one, mimic the occupying variety. Many who do not assent to Mendelian theories believe that mutants are unfixed newly acquired variations and some think that they are in one sense pathological. I note that this is McBride's view, following Muller. Such suggestions seem to fit in with what has just been said. They also fit with

the general view that all variation is in a way pathological since it is a disturbance of settled order, or the result of such a disturbance.

It may then be argued that the likenesses, Batesian or Mullerian, are forced likenesses ; that they are not due to accidental mutations or mutants ; and that they are not protective at all, since it is only in the larval and pupal stages that protection is really needed. That new mutations should arise from new crosses we need not doubt. Again and again the pleasing drama of the chromosomes to which Mendelians attach such importance, will be played out with the labile shuffle of non-specific characteristics, while the stabler, but still not static, cytoplasm carries and yet slowly changes the main construction of the organisms. The view that chromatin when reflexly and possibly electrically arranged in the chromosomes should only then have constructional powers, after the cytoplasm has definitely laid out the main or prime embryonic tasks, must be rejected. The well-known experiments on the ova of Echinoderms and Ctenophora demonstrate the potentialities in the cytoplasm. It is remarkable to note the certainty with which the chromosomes are given all the powers of the cell where we even possess little knowledge of the apparatus of Golgi. I suggest that some enterprising Lamarckian stakes out that territory as a clearing house for acquired modifications.

True and False Mimicry.—To reject mimicry as protective in the case of butterflies on the lines laid down as causal by the Mendelians is, of course, not to reject it generally on Darwinian lines, when those are themselves modified by a Lamarckism which accepts the effects of small modifications if a fair suggestion of their origin, repetition and aggregation can be traced, even though not traced definitely, to life, habit and environment. The earlier slight study of the Vespiform Moths properly employed such additional variations where once accident or disorder, external or endocrinal,

gave natural selection chances to work. This organon may be used to work out the innumerable cases among insects in which cryptic or aggressive mimicry occurs. Are we not in the same case when dealing with mimicry generally as the early Darwinian enthusiasts were as regards their great master? They thought he had solved everything. Probably we have barely touched mimetic problems. We cannot distinguish real from accidental mimicry. There is an Indian butterfly with wings so shaped and marked as to suggest the head of a cobra. Do birds avoid it as a kind of winged *Naia tripudians?* Has anyone yet offered a reasonable explanation of the stages by which a butterfly when seated with folded wings on the twig of a shrub or tree is practically indistinguishable from a leaf? This is usually regarded as the result of pure Natural Selection as Darwin left it. In this case Mendel must seem little able to help, and it is not easy at first to use Lamarck unless he is greatly modified. But though we can no longer accept minute spontaneous variations of unknown origin as valid, we can accept minute aggregations if they accumulate out of habit and habitat. It may be suggested that here the early pre-lepidopterous ancestor was bred up with and upon some favoured tree or plant. It never developed high colour and usually kept in a forestal horizon. It was a "conscious" insect as all living things are, for they all know their business. It knew where its food was as larva or imago. It recognized its enemies. But it belonged not to the excitatory type but to the inhibitory. Pavlov can help here. It was quiet and "cowardly." Alarm inhibited its actions. It thus had to keep quiet, and those that could not keep quiet perished in the jaws of prowling lizards or predaceous insects. It knew that safety lay in not moving more than a leaf would move. Many behaviourists may accept as much. Those will who analyse consciousness into sensation and perception and irradiation of centres not directly stimulated. Nowadays Darwin would in all

probability agree. Much of his objection to Lamarck was, I believe, founded on his dislike of that zoologist's semi-psychological language. For if Darwin's great object was not to eliminate " purpose," as commonly used by zoological addicts to what may be called a morbid semi-religious " mentalism," then am I greatly mistaken. So a butterfly knowing its business, that is reacting with the probability of safety to its various environmental excitatory and inhibitory stimuli, and living and breeding and constantly resting with folded wings in a particular chosen environment, mating there and laying its eggs there, had a darkened habitat for itself and its larvæ and pupæ. Its tendency to inhibition and its probable low metabolic rate may serve to account, on the theory of energy coloration, for its lack of colour and for its rigidity when stimulated by fear. For inhibition is a secondary result of stimulation, or over-stimulation. And still it had some energy to dispose of and it grew in size, as other animals grow, not because slight increase of size is advantageous but because growth, as shown elsewhere, is a normal result of endocrine or pre-endocrine factors. Every variation towards leaf form or colour would, if it followed the line of past variation, be of use. The line followed would be that indicated by the beginning of the line. For when a variation occurs once it has, by the irreversibility of evolution, some indication to follow. A variation shows a metabolic instability in a particular direction, and metabolism is ruled by environmental nutrition and habit. If there is some germinal instability at the back of this we must take it that such instability is caused by the environment. As said before, variation is always on the verge of morbidity, if not actually pathological. This butterfly has a growth simulating a leaf stick. This could not begin by a large mutation, for in spite of T. H. Morgan's belief that large embryonic changes may be Mendelian mutations, the evidence is doubtful, and there is little reason to believe that mutations really affect

actual generic structure. It is far easier to believe that this leaf stalk began as a slight thickening of tissue, vaguely suggesting as it increased something that a lizard working in shadow might mistake for a stalk. Here there is something for natural selection to work on and the problem comes into line with the case of the Vespiform Moth. I leave the question to unprejudiced students of mimicry, but suggest that it was by accidental morbid variations of form that numberless cases of mimicry have arisen. Malformation may be indifferent, harmful or lethal as regards racial continuance, but in the first two cases it may be compensated for by repair and the suggestion of something on which natural selection can work. Malformation in the sense of overgrowth is undoubtedly responsible for many insect extravagances for which faithful Darwinians seek a useful end and in their endeavour vary in equally marvellous ways. The remarkable reasoning employed by some of the most furious and faithful advocates of useful resemblance might afford splendid material for a light and cheerful lecture by a logician seeking relaxation.

Emus Hirtus.—Let me conclude this laborious effort to be orthodox and unorthodox at once by offering an example of extraordinary resemblances between wholly different organisms which may, perhaps, give occasion for thought to the stricter sects among students of mimicry. In an obscure part of England, which I am forbidden to mention, since any indication of its locality would be followed by a rush of professional and amateur coleopterists to the spot and a consequent destruction of this wonderful and rare beetle, there can be found the *Emus Hirtus* (Linn.). When I was shown a specimen by an eminent student of morbid histological variations, who solaces the rare leisure which frees him from the microscope with the study of the Coleoptera, he asked me what it was, and I replied with the rash confidence of the ignorant that it was obviously a bumble-bee. Such a mistake was, however, regarded as perfectly

natural, for even the wise and instructed were momentarily often at a loss to class it. This insect is covered on its upper surface and elytra with a long downy yellow fluff, exactly resembling that found on bees. Its shape, though flattened, is remarkably bee-like. Without a doubt a discerning and intelligent bird, instructed by experience, would say at once, " This is a bee. I must not touch it." Here then is a marvellous case of mimicry. And yet there is a catch somewhere. Underneath the insect is coloured a brilliant cobalt blue. I was much concerned to find out a reason for this which would appeal to professors of mimicry, and only at last decided that if the beetle accidentally got turned upon its back, any enemy would, or might, mistake it for a tiny pool of water reflecting the bright blue sky, or for a ferocious " blue-bottle." On further investigation, however, I discovered that *Emus Hirtus* is wholly nocturnal and that, as an additional protection from those enemies which may be supposed to fear its apian characteristics, it lives almost always under drying cowdung.

CHAPTER THREE

SOME ANGLER FISH

The Ceratioids—Male Parasitism—Tissue Zygosis—Morbid Evolutionary Process—Fusion—Apparatus of Explanation—Angler Fish and Genes—The Cell's Tools—Gelasimus—Flat Fish and Form—Environment and Function—Unorthodox Organisms—Evolution and Stress.

The Ceratioids.—There are other animals than those in one genus of snakes which possess an erectile dentition, and it looks probable that in the case of the Ceratioids, a group of Pelagic angler fish with the first ray of the dorsal fin modified into a line and bait, the power these extraordinary creatures possess of erecting and depressing their three rows of teeth was attained exactly in the way suggested for the origin of this capacity in certain snakes, as their inward curved form makes it difficult to let go. And if this is denied in favour of any form of genetic orthodoxy it is curious to note that other special characteristics of these fishes demonstrate in a most remarkable and almost incredible manner the truth of a form of descent with hereditary somatic modifications. This is so clear that nothing but the frequent ignorance of general zoology displayed by professional theoretic biologists can explain the neglect of the evidence these fish afford. And so far as I can discover no icthyologist, not even Tate Regan, who first described this great peculiarity, has pointed out its real significance in evolutionary theory. But soon after completing the earliest draft of this chapter I found that J. T. Cunningham (1928), had a pertinent passage on the impossibility of reconciling the facts to any theory of genes, mutants or

mutations. Yet, when he says "it is easy to understand how the evolution of the condition could have taken place gradually as the direct effect of the males holding on to the skin of the female in seeking satisfaction of the sexual instinct," I cannot agree that there is in this any sufficient suggestion of the mechanism involved in what has obviously been a highly prolonged evolutionary process. It may be easy to understand for those whose training has led them to regard orthodox theories as a subtle denial of real mechanical explanation, but all who are subdued to the Mendelian dyes in which they work will not unnaturally ask for more if they are to reconsider the problem.

Male Parasitism.—The most remarkable characteristic of the Ceratioids is the peculiar parasitic relation of the male to the female, which was first discovered by C. Tate Regan (1926), and has, perhaps, no real parallel in zoology. We are accustomed to many forms of minute motile males living upon the female's body as parasites, but in the Ceratioids the male not only draws its nutriment from the female but in the adult state is actually fused with her body in such a manner that the general nutritive systems are one, though the male has a functioning heart and his own blood-stream, and has preserved, in some cases ample and in others very minute, mouth apertures for gill function and oxygen intake. This example of what may be called complete parasitic zygosis with an atrophy of the early functional gastric apparatus of the male, obviously means that at an early period the male lived its life as a free organism. This probably was at the time when male and female were somewhat the same size. To suggest approximate stages in the drama as it was played out in the ocean abyss is perhaps not so difficult as it might seem, if a little imagination may be used to elucidate so strange a set of related variations. In the deep sea, apparently so scantily supplied with forms of life, the meeting of the male and female must have been a great matter of chance. I do

not lay any stress on the suggestion but it is possible that the female's ova when fertilized were in some cases actually adherent to her skin. If this were so, and there are many analogies for this, the female's skin was *ab initio* the home and the feeding ground of the early free male as well as a place of sexual attraction. The female was also the male's means of translation from one place to another. She carried with her the means of fertilization. This semi-parasitic life of the male led him to detaching her normal small parasites and to sucking at the wound made by this forcible detachment. It seems that in such a semi-morbid condition the rest might easily follow, and the hypothesis seems the more likely when we reflect that it must have been assisted by the female herself, urged on by the nitrogen hunger which Keeble (1910), in his enchanting little monograph *Plant Animals*, has shown to exist everywhere in the sea, taking to cannibalism, thus reminding us of the habits of many of the Arachnidæ. So far as I am aware this suggestion has not been considered. But if she ate all of her suitors which she could catch and drove away the rest the race must have perished. We are bound to infer that some of them found the only way to salvation was to hang on to her in a position from which she could not dislodge them, try as she would. But it was only when this process had gone as far as complete fusion that the male would be quite safe, and fertilization of the extruded ova practically certain. Possibly the male gonads stimulated the female through the common lymphatic circulation. It would be interesting to speculate on the hormonic machinery by which the sex orgasm of the attached organisms synchronized, but all that can be said of this is that the very fact of a common lymphatic stream may have ensured it.

Tissue Zygosis.—If, then, the male at last found that his easiest method of obtaining food and safety lay in this form of parasitism it is easy to infer that it resulted in the actual destruction of all females which could not by a powerful

local reaction protect themselves from their predatory facultative mate. Those, however, which could so react must have done so by supplying that early parasitic mate and enemy with abundant food, so abundant as to produce atrophy in all those organs which had been functionally brought into being for obtaining nutriment. Finally the male lost all capacity for freedom. In another place (Roberts, 1924) I have worked out some of the problems of embryological zygosis, and those reactions on the borders of pathology found in mammalian gestation (1926), and here it may be suggested that the female ceratioid's somatic reactions strongly resemble such as were treated there. Is it merely picturesque to say that the male at one point of evolution must have acted as a kind of cancerous invading body? Leaving this to others to determine, here I am more concerned to ask if these continuous variations by which a partial male atrophy and final fusion occur can be explained by minute accidental accumulative variations or by any form of mutation? If not, and I consider the affirmative too absurd to be defended and too absurd to attack in such a case, we are clearly faced by an ontological reproduction of a prolonged series of phylogenetic pathological phenomena merging finally in a physiological condition of an unparalleled character.

Morbid Evolutionary Process.—Now here we have a most instructive natural experiment, for it is by relating such an abnormal and monstrous result to phenomena which we are accustomed to see that we can best hope to learn the methods of evolution. It would be idle to contend that what we note in these fish did not arise in a morbid process. It may, indeed, be suggested that in the females the spinous tubercles of their skin arose as a protection against the male. I lay no stress on such a speculation, but in the face of these phenomena we are entitled to make any guess whatever. The main points we are concerned with are the origin and perpetuation of these inter-organic

tissue relations. There is no need to insist on the reasons we have for supposing that the main mechanism of organic evolution is a primitive shaping by function (we may start at any stage) and that, as further re-modelling based on the earlier stage takes place, those early results of function become a necessary preliminary *in embryo* to later modelling. Thus function, open and in action, and function past or preparatory, are the great factors of morphallaxis, and all variations occurring are the result of the stresses of function conditioned by the whole environment. Taking this view we can understand how very small variations do tend to accumulate, a thing that Darwin saw but could not explain so long as he ignored present and past function. It is this way only that we can recognize really advantageous minute variations.

Processes of Fusion.—But here it is of greater interest to discover the process by which these free young male fish fuse with the female. Is it not clear that we cannot regard the process as a fresh start every time? Obviously it is a process perfected by long and profound physiological preparation, an ancient epigenetic piece of machinery. I say epigenetic because each step clearly calls for the next and we are bound to infer that the primitive process has been continuously shortened. The reactions of the male and the female are now definite and presumably devoid of pathological features properly so-called. The path has been found out and is followed. Are we not entitled to say this of all organic reproduction? Those reactions which we call reflex are very accurate. But they can err or be broken as they employ not merely single but complex nervous reactions. Yet is not a reflex a mechanism built on the higher certainties of its simpler units? The further we go back in the analysis of living matter the greater the certainty of right, of correct, action and reply. If it were not so would correct reproduction ever occur? The reactions then of these fishes are, now and at last, those

intelligibly "predestined"; that is, they are bound to occur if the ancient path is correctly and automatically followed. It seems to me that the significance of all this is that reproduction is no longer an utter mystery. It is the failures of reproduction which require explanation. Continually we are referred to pathology to explain variation, either slight or disastrous. It may be noted here that some have suggested that the fusion of the male and female is placental in nature. I do not see the necessity for this, though we can possibly call it placentiform if we regard any connective tissue reaction as analogous to a placenta. For what is the placenta but a highly specialized connective tissue reaction to satisfy the embryonic trophoblast? The references given for other suggestions of my own may possibly be useful here.

Apparatus of Explanation.—Is any explanation of these Ceratioids to be found in modern theories which are supposed to be orthodox? The accumulation of advantageous variations out of variations in all directions is recognized on almost all hands as an insufficient apparatus to do the work. What, then, about it here, when we must postulate so many accidental variations coinciding to establish the new bi-sexual fish? Clearly we cannot swallow any such conceptions if we do not possess an amount of faith sufficient to furnish forth a whole theology. What, then, of Mendelism? Can we assume that the alterations concerned are due to genes, that they are, in fact, mutations? Once more we must ask for karyokinetic accidents and mutants to coincide, and must demand that the losses and transmutations of the degenerate male should be regarded as lost or inhibited genetic characters independent of the environment. Those who are prepared to accept this can accept anything. For here both male and female are the main part of each other's environment. We are then left face to face with the necessity of doing without any cut and dried explanation on orthodox lines. Here, if

anywhere, we have to fall back on definitely attained and inherited modifications which are reached by variations due to use and disuse and pathological states. Lamarck's "need," which I translate freely as the push of energy towards function, always assuming as we may that any function when established implies the easiest path of action open to the organism in question, may serve us here. And, if we like, we can at least say that Darwin's Natural Selection was in such a case a great factor. Those males which did not secure themselves positions as parasites perished, and so did the females which did not react in evolutionary repair. But in this case we are not concerned mainly with such early stages even if we grant that the male choice of action was an accidental variation. What we are concerned with is the growth mechanism which, with all its complications, followed the first step.

Angler Fish and Genes.—And here it may be suggested that *Edriolychnus schmidti* and her nightmare congeners cannot be said to have reached any stage of static development. There is no sound reason for believing that their evolution is any more complete than that of humanity itself. Further changes may be proceeding rapidly, changes, of course, on the lines now definitely established. The male may degenerate further and penetrate deeper. The processes by which his phylogenetic parasitic retrogressions are inherited may be further abbreviated until he becomes no more than an attached sunken gonadal sac in the female. This speculation cannot be called absurd as a definite line is established. The casual uninterested observer might then imagine that this possible future Ceratioid was a bisexual species. Such mistakes have been made by others than the unlearned. The hectocotylized arm of the male Cephalopod was once described as a parasite in the female. If such a speculative view be accepted, or at least scrutinized and not at once contemned, in what way can we look to genes and mutants to explain

the various successive stages ? The genes of the Ceratioids must be endowed with linked constructive and retrogressive powers. As no mutationists ever explain how genes are acquired or come into being, or how, when they occur, they can be lost or inhibited, the phenomena of parasitism are as obscure on the genetic theory as that of general somatic construction itself. Any theory that gives genes a general constructional control would contradict the orthodox Mendelian theory, even if T. H. Morgan has lately stated that a gene may affect any part of the body. Is not that a surrender of the principle of particular genes ? Later I shall comment on a biologist who spoke of " the particles in the germ plasm which represent muscle." And yet he protested that Weismann's *ids* and the like were out of date. This was in 1921. We may also note that Punnett once declared that genes cannot be acquired though they may be lost. Quite lately I have inspected with much admiration T. H. Morgan's *Experimental Biology*. It is a remarkable, a monumental, volume indispensable to those who wish to know anything about the way in which protoplasm works with the tools it has picked up, its colloids and crystalloids, its enzymes, catalysts and electrolytes which started the reversible reactions of that remarkable compound to whose functions we give the name of " life." But however extensive I found my own ignorance I found nothing in this great book as to the origin of genes, nothing of the functions of the cell's working tools, their acquisition or their loss. And very much did I regret to find that the use of such a word as " fertilizin," drawn by analogy from the amazing dictionary of the bacteriologists, was used to " explain " or elucidate the obscure processes of fertilization. So immunologists use other words ending in " in " to hide their ignorance of the obscurities of living colloidal reactions.

The Cell's Tools.—The genes dominate construction and supply mutants. Do they ? It seems that very much

of the work done with such amazing skill and patience really negatives such conclusions. Experiment thought out and experiment accidental seem to show the extraordinary powers of the environment when position and gravitation and other simple factors come into play. That the presence in the cell's armoury of what I was, perhaps, the first to call the cell's " tools " has been due to gradual acquisition, increase and diminution, cannot be denied. If we could learn that these were indeed the genes much might be granted to the theory, but I cannot learn that any geneticist considers that they, and they alone, are the cytoplasmic chromatin becoming visible when aggregated into mitotic patterns.

Gelasimus.—Morgan refers only rarely in this book to adult stages, but in referring to the Fiddler Crab (*Gelasimus*) he remarks that when the big claw is removed it is replaced by a small one—" the induced symmetry has inhibited the normal asymmetry. In other words, when at a critical stage the individual has been led to take a false step, no subsequent rectification is possible." A gene then is only good for one correct piece of growth, or when circumstances change it can produce another. This seems more agreeable to the simpler view of descent with modifications than with the genetic theory. Or are we to conclude that another gene, of less constructional ability, which was " inhibited " by the first growth, comes into play ? Can this be fairly reconciled with the perpetual renewal of teeth in fish and reptiles ? Here there is a gene with unlimited powers.

Flat Fish and Form.—In another place Morgan seems to quote with approval the view of M. Kyle that the forced changes in the young fish are the cause of its taking to the bottom. There might be something to say for this, as Cuènot and myself have argued on the same lines as regards the effect of forced variations compelling, or rendering advisable, a change of habit and environment. But if

this view is correct as regards flat fish how comes it that the genes are responsible for increasing asymmetry, of such a character that the early genetic characters of the young flounder are so forcibly distorted as to correspond with my own view of forced morbid variation. " The eye is pushed over by the growth of the sub-ocular ligament or the prefrontal. . . . When rupture of the tissues is apparent . . . they are forced apart." On the whole, when we consider the coloration problem as well as the changes in form, it seems to put less strain on the imagination if we prefer the views that hiding and feeding on the bottom gradually produced the effect needing explanation.

Environment and Function.—In face of the phenomena of the Ceratioids we infer, perfectly legitimately, that function is the process by which the future path is partly, even largely, determined, provided no new environmental factor intervenes. In the Ceratioids when male parasitism first occurred, there was such a factor and henceforward the evolutionary path was differently determined step by step. For since the process paid by securing fertilization to a very lonely female fish, the male degenerated, and she had, probably with great results, the additional stimuli of the male's gonads. Here we cannot postulate any accident of karyokinesis. Nor does it look feasible to argue that any loss or inhibition of Mendelian factors altered the male. Such an argument is superfluous when function alone can preserve structure. Nor, if the male hormonic influences altered the female, can we argue that her changes were genetic. They were obviously environmental, for the male becomes part of her continuous environment. And when we disregard karyokinesis and Mendelism we are forced back on nothing but environment and function, with the processes which ended in the new fused hermaphroditic monster shortening the evolutionary changes of this new development. It is pleasant to note that the ichthyologist Tate Regan, when remarking on specific

characters, considers many of them due to the environment. And I should be surprised if he ever varies his expressed disbelief in mutations as the foundation of species.

Unorthodox Organisms.—To go further we may say that one such case, if it were one only, is fatal to the orthodox beliefs as to the machinery of development and growth. For it is clear that the female Ceratioid has no business to have her tissues so constituted that under an environmental stimulus they are prepared to join with the tissues of the male in as complete a zygosis as that between the embryonic processes of the human jaw. And it must be equally clear that the conduct of the male at the present time in surrendering not only his independence but also his mouth, which is toothless obviously owing to disuse, is highly unorthodox since, owing to the environment, it follows a course of rapid prepared degeneration which must have taken ages of inheritance to bring about slowly.

Evolution and Stress.—If when writing my original paper on *Repair in Evolution* (1920) I had been acquainted with the life history of these Ceratioids it would obviously have found a place there. Though I am as firmly convinced as ever that in the anatomy of the human body alone sufficient proof can be discovered to show that variations of actual structure, which are not Mendelian, since Mendelian mutations are not really specific characters at all, always or almost always arise as the result of disturbing stresses, it is open to those who deny it to affirm, even against evidence, that they are the result of Darwinian minute advantageous variations. To say so of course implies that there is no definite cause at work, for the essence of Darwinism is here pure accident. But is it possible for any, however much they are wedded to accepted doctrine, to say that the clearly consecutive and correlated changes of these male and female fish are accidental or "spontaneous" or the result of coinciding mutants? Difficult as it is to imagine any definite structure arising

from anything but continued functional changes, both surface or deep, that is, from function present and function past, as one form of orthodox theory demands when it postulates variations in all directions, thus ignoring the negative value of equally likely useless variations, or when another gives us ready-made genes which supply lucky mutants, it must surely be owned that what is an insuperable difficulty to many becomes here so immensely increased that none surely can undertake to swallow it.

As more may be said later of the genetic theory I shall leave it now, merely remarking that no zoologist or biologist, however labelled, has ever denied that there are, of course, tools, of whatever kind, which mainly determine established embryological growth. It is interesting to learn what happens during that period, but we do not learn from this study how the germ tools get changed. If some able geneticist can " explain " on a pure gene theory the problem of the Ceratioids, something will have been achieved.

CHAPTER FOUR

PRINCIPLES OF LAMARCK

Defenders of Lamarck—Pioneer Work—First Proposition—Second Proposition—"Interior Sentiments"—Third and Fourth Propositions—Lamarck and Mind—Ribbon Fish—Excursus—Interest in Science—Suggested Investigations—Lamarckian Proof—Direct Proof—Variation in General—Function in Evolutionary History—The Giraffe—Roux on Functional Stages—Breakdown and Repair—Evolutionary Operations—Pathological Variation—Mechanism of Heredity—Lamarck and C. M. Child.

Defenders of Lamarck.—As part, at least, of the apparatus of explanation used in previous chapters was drawn from Lamarck, it may be as well to suggest how far his principles should be employed. We need not deal here with the able general arguments based on theory and experiment with which Lamarck's views have been defended by McBride, Cunningham and Dendy, to say nothing of others. I shall therefore merely try to show in what way the four principles, drawn from *Histoire des Animaux sans Vertèbres*, as they are put more clearly than those in the *Philosophie Zoologique*, have in this book been interpreted and altered by means of addition and subtraction. Had I not considered that some changes were absolutely necessary it would have been enough to rest on Dendy's *Outlines of Evolutionary Biology*, perhaps the clearest and most adequate textbook on the subject. We may judge from many of the arguments used by opponents of Lamarckism that they have not read Lamarck's books, while some of his very defenders use others which that zoologist, expressly or by strict implication, would have considered devoid of relevance.

These are cases in which the results of somatogenic variations are supposed to be obvious in a very short time, whereas he insisted on a great number of generations in a continuous environment. Nothing need be said of the supposed refutations of this zoologist based upon the non-inheritance of mutilations. Relying on his general defence of the Four Principles he would have declined to reply to critics who used such arguments. His insistence on continuous function over great periods of time is sufficient.

Pioneer Work.—How unjust it is to refuse early investigators any credit because all they said cannot be admitted literally may be left to unprejudiced critics to determine. An endeavour should be made to interpret them on lines which take into consideration what they would probably have said if later work had been known to them. Otherwise we might deny any value to Anaxagoras, or to Lucretius, to say nothing of later pioneers of evolution. That the definite statements, the conditioned arguments, of such pioneers should be at once capable of translation into the language of our time is not to be expected. They should be looked on as suggestion. Lamarck was undoubtedly a genius, but not even geniuses can anticipate all future knowledge. No one need expect all he says to be at once clear and obviously correct. Very naturally Lamarck was often crude, since the very exuberance of his zoological imagination made him hasty and sometimes over-fanciful. If this is often the mark of the unorthodox some may deem it more helpful and hopeful than the "lag," technically known as hysteresis, which is frequently the failure of the accepted schools.

First Proposition.—Lamarck's first proposition runs: "Life by its proper forces tends continually to increase the volume of any body possessing it and to enlarge its parts up to a limit which it brings about." Those who have concentrated on the question of use-inheritance rarely mention this first principle, though it is of great importance.

But it cannot be denied that such a proposition or rather such propositions do not at once command consent. Elsewhere when discussing size and growth I have shown reasons for believing that the main machinery for the increase of growth lies in the action of the endocrines with their qualitative and quantitative results which are stimulated by their opposing products into neutralizing each other's toxic products, or excretions. This extension of the principle of immune action, though new, is clearly in accordance with the views of Lamarck, who was induced to make his general statement by the observation of zoological facts. The rise of the various equine species from such ancestors as the *Eohippus* is an example now known to everyone. That increase of size has been due to advantageous minute increases in growth cannot be accepted in view of the facts of overgrowth, sometimes monstrous, sometimes incapacitating and sometimes dangerous to the animal concerned. It is needless to give examples. Clearly Lamarck could not at that time analyse the " proper forces " of life, but just as clearly he was justified in stating the facts as he saw them and attributing the results to the way life acted in the environment in which it found itself. We take it, of course, that the word " proper " was deliberately meant to exclude any external " force " or " power." That it excluded any mystical element of an intrinsic director is obvious on account of the whole argument as to environment and the natural energies of the organisms concerned, though as stated later I think Darwin, in his obvious desire to set evolution on a purely mechanical basis, was suspicious of the psychological terms sometimes used by Lamarck. It seems to me that there is nothing so far which Darwinians could not accept. Apart from special cases of degeneration or of a singularly static environment, they must observe that animals do tend to increase in size, or volume. And thus they tend to enlarge the parts, however the enlargements are distributed.

But a very close critic might well demand an explanation of the words "up to a limit which it brings about." As Lamarck, so far as my memory serves me, did not specifically interpret the saying, we are justified in examining it to see whether it is well founded on what we now know. Any limit may be imposed by the environment. An animal living and growing in a swamp cannot increase without self-destruction to a size which prevents action in a morass. If we said that living in such conditions was probably one factor in the evolution of flying reptiles it would be a legitimate suggestion. The weight of the mammoth in the Siberian tundra may have had a lethal race result. But I do not think Lamarck meant this. "The limit which life brings about" is the limit imposed by the total amount of energy which the animal can extract from its surroundings. No doubt this is saying almost the same thing as that said of the mammoth and many extinct giant reptiles. If these had not been destroyed they might have grown until want of surplus nutritional energy stayed the process. Life, by a legitimate process of logical reduction, can be said to be energy. We may note, by way of illustration, that engines of human manufacture mainly grow in size with the work required of them and the energy supplied to do that work. Lamarck does not appear to have been thinking of the destructive power of the environment but of lack of energy. There seems to be no sound reason for disagreeing with him if we neglect what was not known to him, and is still under discussion: I mean those heterogonic differential growth ratios which have been mentioned elsewhere. In these cases the limit is imposed by loss of balanced growth. But animals usually increase in size so far as their nutritional energy allows them to do so, without loss of symmetry and balance. Whether the mechanism of intra-organic immunity, such as here suggested, be correct or not as supplying the necessary causes, or stages, in growth and over-growth,

may be doubted, but it implies that when the tissues of the animals concerned are specialized, their excretions or secretions must be met and dealt with by every other tissue of the body. When endocrines are developed in the ductless gland system they influence each other and must live in endocrine balance. Physiologists are aware, though little has been said of the subject, that these glands not only act as regulators of metabolism, but also as regulators of construction. They act not merely qualitatively but quantitatively, as the phenomena of acromegaly so forcibly suggest.

In such a general statement as regards Lamarck's first proposition there seems to be nothing to which a modern Neo-Darwinian, not given to any form of concealed orthogenesis, need object. Natural Selection is obviously implied and the Neo-Darwinian may even use Mendel and employ genes and mutations or mutants as he will, however other schools of thought regard them.

Second Proposition.—It is in the second proposition that really contentious matter may be found. "The production of a new organ in the body of an animal results from the interpolation of a new need (*besoin*) continuing to make itself felt and a new movement which this want gives birth to and encourages." This obviously implies the inheritance of somatic modifications but it need not be regarded as more than a mere skeleton of what was in Lamarck's mind. The full meaning of the proposition may be put thus: "Changed conditions change function by increase or decrease of special actions directed to the continuation of life. Such changes may be described as the result of use and disuse. Inheritance takes place by slow changes in the metabolism and axile gradients of the embryo and is purely epigenetic, being entirely dependent on the continued effect, generation after generation, of the environment making the altered functions answer to the new calls of life in the new environment."

So far as Darwin is concerned I do not see how he could have objected to this, seeing that he grew even keener on use and disuse as he grew older. And he could, without any grave difficulty, have reconciled his pangenesis theory with it. For pangenesis can be described as a rough and rude forecast of hormonic and catalytic heredity which again implies that new catalysts may be picked up and new hormones developed.

Interior Sentiments.—Lamarck's views as to the "interior sentiments" of the animal in the creation of a new organ seem to have referred, or at least mainly referred, to the exterior organs seen in use. If he had known as much, or as little, as is known now of the endocrines he might have extended "les sentiments intérieurs" to the new needs of extended glandular and general tissue change, as new stresses and stimulations came into action. This view of the internal environment and nascent functions is now familiar to all, however they regard it, but I do not see how Lamarck could have rejected the conclusion that they too depended on the whole functioning of the animal as an organism dependent on the environment from the start and built up gradually by the inheritance of ancient forced function and consequent new variation. I note that Russell (1916) feels sure that the *sentiment intérieur* of Lamarck is "a psychological faculty or it is nothing." I believe Darwin made the same mistake. Lamarck must be interpreted in the light of our candles as well as of his own. He deliberately put aside real psychological factors when he states, as Hunter did, that the mind is the brain. His "nervous fluids" are, after all, nothing but nerves. But he had to use the language he possessed. There is nothing more unfair to any investigator than to seek in the obligatory obscurities of words, not yet obviated, contradictions to the worker's whole attitude and method of thinking.

To what extent Darwin or Lamarck would have assented

to the doctrine of stressed function, breakdown and repair as leading to variations and further modification in growth and function I cannot venture to say, as neither of them appears to have used pathology as a tool of analysis. But since this hypothesis implies new functions leading to new developments it is consonant with " use " and, as it implies a new direction of energy, it also implies or at least frequently implies, a concomitant " disuse." There must be an evolutionary continuous series of inter-dependent changes of structure as what had been used in one way becomes used in another. Those instructed in the merest rudiments of embryology will be able to supply numberless examples of this. In any case both these zoologists could have ignored the pathological side of the changes while they fixed their eyes on the continuously caused alterations of Lamarck, or on the accidentally accumulated alterations of Darwin, which were all to the good until overgrowth occurred. Neither, however, went into this question which is still so little understood.

One criticism frequently made of Lamarck's views is that if they were correct evolution would be very rapid. I take it that this is due to any possible standard of rapidity being wanting. And *per contra*, it might be urged with just as much, or even more, reason that on Darwin's scheme of pure accident evolution would not have happened at all. But here we have the one great difficulty in understanding Darwin. Did he ever recognize that use and disuse, however interpreted, implied a definite line of change by means of variations, of which the origin was thus clear, which did not fit with his variations of unknown origin ? Curiously enough those who discard half Darwin are the most angry if it is suggested that at times he showed a remarkable lack of clarity in thinking.

It is now obvious that the whole struggle turns on the question as to whether somatic functions, however slowly, enforce divergencies of chemic action, or chemo-differentiation,

on the embryo. As as illustration we may ask whether alterations and repairs, following upon stresses, in the "shops" of a great railroad, are reflected in the head draftsman's office? It is clear that they must be, for the actual working of the locomotives and rolling stock under pressure is the railroad's trial ground. As I have showed elsewhere the evolution of social or juristic law follows similar lines. These are in fact general laws. That Lamarck would have admitted them to be such I have no doubt, though there is no evidence that he recognized them in action among societies of all kinds. If he ever read Hobbes, the first great, if very crude, biologist, there is no evidence of the fact.

If Hobbes was crude so was Lamarck. So was Buffon. Every pioneer in thinking is likely to be. We must judge them as people of their own periods. It is utterly unfair to Lamarck to state that a "new want" immediately produced "a new organ," and yet that is what many try to put upon him. He must be interpreted in the light of all modern biology. And it is very doubtful if any modern biologist really lives up to his opportunities.

And here it may be added that the hypercritical mind, being necessarily narrow, usually selects the crudities and possible absurdities of any pioneer to whom it is hostile, instead of judging his work as a whole and considering the period in which it was done. Hippocrates made mistakes and was bound to make them, but no modern physician scorns him on that account. No surgeon who knows his business can read him on fractures and dislocations without admiration in spite of modern advances in bone surgery. And it is probable that those who regard Paracelsus merely as a quack have never sought for merits in him.

Lamarck's third proposition may be translated: "The development of the organs and their capacity of action is in a constant ratio with their employment." Very little comment is needed here. Somatically it is true so long as the action required does not lead to over-stress. But

Lamarck obviously meant here that continuous function in parents led to increased power in the offspring and that continuous local relaxation of function was reflected in them by a decay of local power. This is nothing more than use and disuse and all that has been said of that applies here. Darwin implicitly agreed.

The fourth proposition states :

" All that has been acquired, laid down or altered in the organization of individuals in the course of their lives is conserved by generation and transmitted to the new individuals which come from those who have undergone such changes."

If Lamarck had written : " Many somatic changes in the individual, due to environmental changes of all kinds which are continuously affecting offspring as they affected the parents, tend to be reproduced by similar functional means in new generations and tend gradually to become hereditary owing to embryonic, endocrine, and nutritional changes which are due to past functioning," why then, it is probable that some, at least, would agree with him. It seems impossible to believe that he means us to infer that all new, or altered, somatic characteristics were directly and at once inherited when we note his insistence on the time factor. And yet H. Elliot, in his introduction to the translation of Lamarck's *Philosophie Zoologique*, says what we want to prove Lamarck right " is one indubitable case of use inheritance taking place under our eyes." I hesitate to characterize this. But to return, " all " is an over-strong word, and I can only interpret Lamarck as meaning that when we do see changes repeated in following generations owing to need (*besoin*), stress, and so on, these changes will tend to reappear even without the aid of repetition of function or stress. That this, however caused, does happen we may be sure. But it seems that it is this proposition which has led to trouble, and also to what, on the lines of the whole argument, may be looked on as waste of time

by Neo-Lamarckians. If "all" that is acquired by one individual is transmitted at once then obviously newly acquired pigmentation in a parent lizard should show at once and without exposure in the offspring. Or at least it should tend to appear earlier and earlier generation by generation. But again, what of the time element? A salamander has acquired his whole constitution during a period beyond our computation. It is true that some show an instability of constitution due, most probably, to their having only recently, speaking here of biological time, acquired the power of acting as land animals, but even so it may be regarded as more or less of a fluke if offspring of theirs show very definite signs of altered heredity after exposure to different conditions during a few generations. We cannot as a rule look for such changes to reappear even a little earlier than usual without prolonged experiments lasting far more than a human generation. Probably Lamarck would now delete "all" and phrase his proposition in a manner more consonant with scientific caution. As regards the interpretation put on these four principles I am convinced that on the whole it would have met with the approval of Buffon, Lamarck, Erasmus Darwin and, seeing how Darwin tended more and more in later years to a belief in use and disuse, even of Darwin himself. Those without prejudice may at least feel certain that he would be very uncomfortable among the newer school who use his name, and also among those who are so controlled by words as to attribute evolution to what they call "mind."

Lamarck and "Mind."—In another chapter I shall have something to say upon this particular subject. Here it may be remarked that Lamarck was bitterly opposed to the introduction of "that curious entity known as mind," which he regarded as purely "factitious." When writing of the nervous system he says about "souls," "All that we can ever say of this subject is baseless and imaginary." He did not follow Descartes and there is little doubt he

thought the very notion of a soul baseless. Again he says, "I started with the conviction that all acts of intelligence are natural phenomena and hence derive their source from purely physical causes." When we see how many Neo-Darwinians and geneticists are obsessed by "mind," it is easy to see why they revolt from Lamarck. That he anticipated Darwin's *Descent of Man* is clear, as he ends his practical proof with, "Such are the reflections which might be arrived at, if man were distinguished from animals not only by his organization and if his origin were not different from theirs," a passage most obviously written with his tongue in his cheek. Buffon had to recant his expressed opinion on this subject. The Sorbonne was too much for him. In spite of these clear indications of his real opinions there is little doubt that much of Lamarck's language can be interpreted if somewhat strained as psychological in the modern sense, and perhaps as vitalistic. That he was fundamentally a thorough-going materialist none should doubt. But many interpret, quite wrongly as I think, his words when they should have read them in the light of his habits of thought. I see no reason whatever for making "sentiment intérieur" anything but the mass reactions of an organism. I note with some surprise that E. S. Russell (1916) does not see how the "subtle" or "nervous fluids" can be directed by this "feeling" (sentiment) to parts where new organs are needed. Lamarck had not the knowledge now in existence of the nervous system. He used clumsy enough words, it may be acknowledged, but he was right all the same. Mass feeling directed into nervous paths could force function and pour energy into plastic areas, thus building new structure. So "les sentiments intérieurs" are in no way properly psychological. We are in fact reminded of John Hunter's very words when Lamarck says, "I have no term to express this internal power possessed not merely by intelligent animals but also by those endowed merely with the faculty of sensation." So John Hunter

said, " I have no word for it," when he came into a wordless territory on the borders of knowledge. There can be no doubt that Lamarck would have welcomed Pavlov with enthusiasm, so little was he a psychological zoologist. It is a fundamental error to describe the doctrine that function creates the organ as vitalistic.

It must, however, be admitted, as I have said elsewhere, that Darwin did thus misinterpret Lamarck. His violent reaction towards a positive and unalloyed materialism made him revolt against the shadow of psychological interpretations, for which his predecessor's words often seemed to give occasion. He forgot that Lamarck, who had shaken himself free of all the dominant theories of his own time, a stupendous feat for any man, had not his own advantages of a deeper and more exact scientific education. To rid the mind of all prepossessions due to Cuvier and other morphologists and to attempt a scheme of progressive functional co-ordination was a task for a giant. It is amazing to think that this has never been properly recognized. Whatever Lamarck's errors were, whatever the inadequacy of his terminology, his undoubted recognition of the power of function, and also of energy, however obscurely phrased, should have rescued him from the shadows to which he has been so unduly and so ignorantly relegated. He who was so essentially not a morphologist gave morphology a new basis. I have seen it said that Darwin really founded modern biology. When employing use and disuse he was a partner, if not a follower, of his predecessor. For once granted these, whatever functions we allot to pure Natural Selection, we have the transmission of " acquired characteristics " or descent with modifications. But not in physiology or embryology, descriptive or experimental, or in comparative physiology, or in anatomy and comparative anatomy by themselves shall we find the key. That lies in the use of a comparative biology in which all the sciences work together.

Ribbon Fish.—Why is so little use made by Neo-Lamarckians of the thousand facts in zoology which seem fatal to Neo-Darwinism and that doctrine *cum* Mendel? In other places I have suggested a few. Here these may be repeated and others found to add to them. One case which has so far not been used is that of the ribbon fish. In these *Trachypteridæ* the initial absurdity of asking us to believe that a variation in form and function must be favourable to be inherited, comes out with force. We cannot always say that a seemingly dangerous structure may not be of use, but there are cases and this abyssal ribbon fish is one, in which such a statement would obviously be ridiculous. The caudal fins of the *Trachypteridæ* are sometimes lacking, but when they are present they are almost always abnormal. They are not mobile but fixed. They are not fixed in the longitudinal axis of the body but are turned to the right. In the language of seamen we may say they have "ported their helm," their rudder is "jammed over to starboard." When they swim they are thus compelled to turn in a circle to the right or to compensate for this forced line of action in some other way. This is not their only peculiarity. The young fish have fin rays of extraordinary length, often twice the length of the body. It may reasonably be argued that during late evolution Natural Selection has eliminated these structures with the result that adults are without them. But how shall we account for the young fish possessing what is, on that view, quite clearly a very dangerous growth? I cannot here go again over the whole ground of normal and abnormal growths dealt with in *Malignancy and Evolution*, but the facts here are consonant with the suggestion, and the proofs of the suggestion made there, that the energy of the body can run into all kinds of fantastic and dangerous overgrowths, which may not for a long period destroy those in which they occur. I pointed out, as Darwin himself recognized, that in some forced change of environment, such a growth might be made use of, and we were then

liable to the delusion that it was an adaptation. This is sometimes used as a " proof " of the rôle played by Mendelian mutations. T. H. Morgan uses it to explain the twisted heads of the flatfish. In this case the fallacies of such an argument are obvious. These remarks as to overgrowth apply with singular force to abnormal tracheal growths such as are found in the emu, crane, trumpeter, the howling monkey, and some frogs with abnormal croaking pouches. No comparative anatomist with a competent knowledge of physiology and pathology would deny that these must have arisen from tissue failures and breakdowns. The trouble is that we cannot get these busy workers to treat such general questions. It may not be far from the truth to say that when they do think of them they regard the theoretic biologists with a mixture of contempt and amusement. They are, perhaps, not sure that the whole subject as they see it treated is not merely a pleasant verbal game for gentlemen unengaged in a really serious pursuit such as comparative anatomy. I hope dimly to be forgiven if I say that my own light but prolonged studies in pathology sometimes tend to the same conclusion. The puzzles set and solved by Mendelians, for instance, often remind me of chess problems, which, however amusing or entertaining, have no real relation to the great game of chess. I venture to commend to them as a suitable playground the subject of Convergence. For, if any can believe that the flying squirrel, *Pteromys*, a placental, and the flying phalanger, *Petaurus*, a marsupial, attained constructional likeness by the coincidental concurrence of similar mutations rather than by inherited functional modifications they should experience no difficulty in believing they can explain the celebrated creed of St. Athanasius. In this case to call a mere Lamarckian speculative is only less than an insult because to do so would be positively humorous.

Excursus.—It must be clear that nothing would have made

anyone attempt a book of this order if he could have found an advocate of a reasonable eclecticism as regards Darwin and Lamarck. Samuel Butler's theism ruined his biological thinking, while his manners prevented him from obtaining any quick recognition of what was good as far as it went. His bitter dislike to Darwin and the essentially psychological trend of his mind, which he shared with Hering and Semon, made him incapable of judgment. Prejudice and eclecticism do not make a good pulling team. But it is not a little disheartening to find that good manners do not always mean good judgment. Men of science, and those who are interested in science, do not, as I said before, now assault each other with verbal brickbats. What we must complain of is their neglect of obvious and righteous implements of war. If the Neo-Darwinians and geneticists are to be blamed for not using all the biological sciences, what can be said about the equally obvious fact that the Neo-Lamarckians are almost as neglectful? Is it legitimate to compare both schools to two species of *Trachypteridæ*, one with the rudder "jammed over to starboard," and the other with it "jammed over to port"? Cannot we see them turning in Mendelian and ante-Mendelian circles? Using another maritime metaphor we can see many of them navigating their craft by the help of an antique cross-staff and dead reckoning and an occasional and inaccurate lunar, when they might have a good sextant and a chronometer and work by the stars they ignore. These controversies have become too much like games with set rules. There is a fight and no one is killed, and after a bloodless battle with clubs the combatants retire into winter quarters.

This is, perhaps, an unjustifiable divagation. But the critic may find that the side reason most inclines him to is as little conscious of the strength of their case as their opponents. I may be pardoned if it is said that I count Darwin as far more a Lamarckian than a Neo-Darwinian. His Pangenesis, as was said before, is as close a shot at what

we may call " hormonics " as knowledge then enabled him to make. He understood function acting and function withdrawn and structure then failing. The minute favourable variations, which, as he put them, are a failure in explanation, are seen to be real factors, as soon as the organic capacity for repair is recognized. So let us hang above Darwin's honourable tomb the weapons with which he fought. I do not recognize the Neo-Darwinian ghost, and if I lament its pseudo-worship, equally do I regret the stubbornness of those Lamarckians who do little more than repeat themselves and their experiments.

Interest in Science.—Another excursus may be pardoned. There are ten thousand intelligent if not specially scientific people in this country who are intensely interested in these questions, or who would be if something like freshness was introduced into them. The millions of fundamentalists can be ignored. They do not count intellectually. The few with brains of a kind can be relied on with the greatest confidence to stultify themselves, as we saw lately after the meeting of the British Association in 1927. When we consider the general attitude in Great Britain towards science it seems an immense pity that our scientific discussions should so often be so sterile that even the intelligent pass them by. Naturally the work of specialists cannot be made popular, unless some particularly exciting fact emerges. If it does we usually see it wholly misinterpreted by ignorant journalists, one of whom was capable just lately of declaring that a notable person died of " meningitis of the stomach." But we might hear to our advantage far more of general popular biology than we get from a presidential address once a year. The only sound basis for real education is a healthy intellectual curiosity. Yet " hope without work draws nectar in a sieve," and intellectual hunger goes unsatisfied. This is the more to be regretted as there is in the memories of those unsatisfied ones to whom I refer a vast amount of useful and wholly unco-ordinated knowledge.

It is not every mining engineer who is a Bates, but there are great numbers of men who might bring in tribute. The moment it is known that anyone is interested in biology, or even in some special branch of natural history, he is often bombarded with questions and given information which he might have sought for in vain in the library of the Zoological Society. Let me give one odd observation which happened to be my own, as an example. Having once an hour to spare near Mafeking, I spent it in turning over rocks upon a little kopje. Under every rock I found from one to five scorpions. As soon as the light fell upon them they all tumbled over on their backs and, holding up their claws, brandished their armed tail between them. Up to that time I had never seen scorpions save in the conventional attitude and can find no one to confirm my own observation. Why did the scorpions turn over in that way? I ask the experts, if they can believe me. These scorpions seem to have acquired a new and unlooked-for set of reflexes.

Suggested Investigations.—And so we come back again to the Neo-Lamarckians. With time and labour and a long rake we might get together a "corpus" of Lamarckian reasoning brought up to date. It is said that "with bread and steel we may get to China." *Hoc opus, hic labor est!* It might be done by some young, ardent and persevering disciple of the great Frenchman, and to him I leave it. "For some there needs must be." I do not suggest that my theoretic young man should employ all his time to the co-ordination of others' work. If he were fit only for that he would not be the type of student to be preferred. He would certainly come across unsolved problems and the queerest facts asking for explanation, and he might try to see if he could suggest a Lamarckian explanation for them. For instance, how came crocodiles and alligators to possess no more than a tongue bud? By what series of events did the parrot's and the toucan's upper mandible become such a perfect hinge? Why do animal races die

out when we are told they grow up by the acquisition of favourable variations? Is it true that Mendelian "longs" and "shorts" are clearly divided? Why does T. H. Morgan now admit a *gene* may influence the whole soma? Why is a bird's right ovary useless? Why are their lungs adherent to the pleural wall? For what reason is the elephant's lung attached to the pleura by loose adhesions? Why has the *Dendrophyllum pertusum* holes in its leaves? How comes it that the equine *panniculus carnosus* is atrophied where a horse's normal tail can flick off flies? He might, when he replied to these questions, go into anatomy and histology and discuss the question as to whether abnormal stresses played any part in evolution. He could not neglect embryology or palæontology and after a while might inquire whether social phenomena threw light on these problems. He would then be forced to pay some attention to pathology and in it would find much to interest him, however he had been brought up. For, as I see it, he would by that time have, on his secure foundation of early training, some inclination to judge for himself. He would in fact be almost educated sufficiently to be a reasonable critic of biological questions, provided he had the rare power of discarding prejudices and of thinking for himself. It is therefore necessary that he should not be sterilized by the schools nor be led by authority to believe that any biologic problem is "chose jugée." A good example of this is the use often made of the somatic tools of the hive bee worker. We have been told *ad nauseam* that here for a certainty there can be no Lamarckian descent as the queen bee fulfils none of the worker's functions. More consideration on the part of Darwin might have saved him the trouble of elaborating his painfully inadequate explanation of hive or community selection by which "accidental" somatic variations were perpetuated. The bee's somatic tools were obviously mainly developed when bees were solitary and communities not in existence. And

furthermore if Dzierson was right in 1845 in stating that all the drones arise from the unfertilized eggs of workers, as some certainly do, and if at frequent intervals in apian history a queen has had to be bred from a working larva already partially developed, there is at hand a real means of hormonic descent with modifications. It is merely necessary to assume, as we certainly may, that some of the hormonic directors of construction thus come from the workers and must tend to influence the eggs of the queen, who has been fertilized by their drone offspring. The queen is thus far from being shielded from all environmental and functional stimuli. In zoology and biology there are few settled questions beyond the fact that evolution has taken place, is now in progress, and must continue.

Lamarckian Proof.—After this it may be excusable to repeat once more that the Lamarckians, of whatever species, do not live up to their opportunities and have none to thank but themselves for the results of their scientific "laches." We may therefore turn again to the subject of "direct proof." I have made but one slight reference to those proofs of Lamarck's amended theory which they look upon as direct. Again it must be said that this is not because I underrate the value of work done by McBride, Dendy and others, to which references are given. But when we contemplate the vast periods of time during which these changes which we call specific and generic arose, does it not seem that we human ephemeræ ask too much of Nature when we seek to prove by a minute's demonstration that which took almost an eternity to work out? Could we go to a great mathematician and ask him to prove in an hour results that had taken him a lifetime to reach? Personally I think experiments have given suggestions of some real value. But after all a "proof" only gains true life when it cannot be controverted and may not be disbelieved by any under penalty of being considered a fool. I do not think the experimental Lamarckians have gone

quite so far as that in spite of Tower and others of whom something has been said.

Direct Proof.—But what is a direct proof? I am told that nothing but a series of cases, such as salamanders, say, undergoing environmental changes which are clearly inherited without the offspring experiencing like stimuli, can be considered such. This assertion I hope to show reason for denying, but in the meantime it may be said that though such cases may occur and have occurred, to ask us to regard this as perfect proof is to ask altogether too much. When we consider that it is in a reasonable form of Lamarckianism that we may confidently use even the most minute variations in quantitative and qualitative chemical changes, since, on the theory, these should follow an indicated line of definite functional accretion, it is rash to risk almost everything on the assumption that macroscopic external variations of an hereditary nature can be secured by a few trials. The true Lamarckian position now is, that while the environment remains the same, clear signs of embryonic changes or new youthful marks or characteristics must not be looked for until a great and wholly indeterminate period of time has passed. I believe this to have been the position of Lamarck, who was obviously not the fool some, and among them Napoleon, took and take him for. But let me ask again what is a direct proof, or the kind of direct proof we may reasonably search for. It is now years since I drew attention, and so far as I am aware I was, after W. Woodland (1903) the first to do it, to the obvious anatomical and embryonic demonstrations of "descent with modifications," in the mammalian testes and scrotum, and to state that the facts were clearly the result of what would undoubtedly be called disadvantageous variations. This Woodland would not have agreed with as he believed the external position advantageous. That such disadvantageous variations could possibly be a factor in evolution was naturally

enough instantly denied by some. What did Darwin say? That those variations in the least disadvantageous would be instantly wiped out. The attempt to introduce pathology into the apparatus of the biologist was on the face of it absurd! A great physiologist told me that he always looked on pathology as a science dealing with states which led to death, but he had to reconsider the matter when asked to reflect on the rôle of repair. Let us draw some further conclusions. There is no such thing as an organism in perfect health and pure physiology therefore deals with the unknown, or only with parts of organs or animals functioning without obvious disorder of any kind. True that pathological conditions can carry us all to death. "We die daily." But we live what lives we can by the powers of the bodily repair always and for ever in action till the final failure of some essential function. If that is so, are there, say in the mammalian organism, any signs that failure and repair must be held to be true factors in evolution? I shall not here labour the point again but may be allowed to quote from earlier work and to make such further comment as seems necessary. In *Warfare and the Human Body* I discussed the evolution of the heart and other organs and the peculiar phenomena connected with the descent of the testes, and thereby gave strong evidence of the part played by breakdown and repair during evolution. The conclusions reached are roughly summed up in the following passage:

"After reviewing phenomena such as these, the conclusion seems inevitable that single small favourable variations have not done the whole work of evolution. They may play their part as correlated changes: but they then take their place in a series of which the causes can be recognized. In combination with reasonable views of use and disuse, and of increased or decreased blood-supply, they may, perhaps, be held to explain such phenomena as the delicate coaptation of some cardiac valves. Their

place in the explanation of the phenomena of mimicry seems obvious. But though they may help us to comprehend how tissues become finished structures, if they are combined with the results of functional energy, they yield no hint as to great or decisive developments, and the mechanisms involved in them. If the reasons adduced for the thesis laid down carry any weight, it is obvious that many, if not most, of the really decisive variations in all internal structure depended, and still depend, not on variations which can be called favourable, but on those that for the major portion of the organisms involved are directly disastrous: not on variations which are small, but on those which are big enough to be appreciable as the cause of immense functional and structural results; not on changes which can in any sense be called spontaneous, by which we may suppose are meant those no cause can be assigned to, but on variations which, though they occurred ages ago, were obviously due to the very same causes that the pathologist can demonstrate to be working at the present day. Only such organisms as respond by direct reactions in a manner that finally turns out to be useful, or at the very least compatible with life and reproduction, are able to survive. The whole of growth and development thus becomes largely a function of effective morphogenetic repair to organic failure and disease."

Variation in General.—And here, perhaps, something more may be said of variation in general. If I am right in believing that "adaptations" are often not adaptations at all in the accepted sense, but that they are forced changes which compelled the changed animal to seek an environment in which it could live, is that a total contradiction of Lamarck, as some might say it is? I do not think so. The Lamarckian variation is due to a change of energy direction forced by the environment. This variation will be improved by function, while Natural Selection will eliminate those not responsive. But if the views advocated

above have any force then additions to the original Lamarckian variations will constantly be made by forced continued growth. These additions are not properly Lamarckian. Thus growth in size, which is a disturbance of settled genetic " habit," will tend to continue " of itself " if the organism has spare energy. It will end in forcing the subject of this kind of growth to change its environment if it can find one in which increased size or change is at least not detrimental. The changes are thus " made use of," and may again be deflected into some other channel of variation, which may, or may not, be increased. Function and the continuous secular changes of the environment may, so it can be imagined, switch over from one kind of variation to the other. A social illustration may serve to show this. A migrant tribe, forced on by the *vis a tergo* of other tribes pressing south, takes to rude boats and seizes upon some island. These boats are the protophase (*anlage*) of a fleet, a fleet growing by small degrees under fear of following invaders. These small degrees of growth are variations, not Darwinian but clearly Lamarckian, as they spring by need and continue by function and inheritance from generation to generation, and continue growing " to a limit," which limit will be determined by the necessity enforced on the tribe and its resources. These variations are what I venture to call " morbid," so far as they are due to stress and are continuously disturbing. But presently when a static condition of the fleet seems to be reached it will be found, and it is actually found in history, that the forces set in motion to increase the fleet go on working until the sea power of that island is beyond its ordinary needs if there is spare energy. These latest variations are then of another order. They represent the continuance, the inertia, of the energy turned in that direction into increasing structure. They are not due to " need " but to " life's proper forces " again asserting themselves in a pseudo-orthogenetic line. With this

forced growth and the growth of population the fleet is at once a weapon of offence and defence, aggression and migration. If the population and power of the tribe increase there will be conquest and colonization. The earlier overgrowth of the fleet, originating in true need, will enable the tribal organism, now hampered on its narrow island quarters, to shift its base, its scheme of life. The overgrown fleet in a larger environment will be no more than adequate. If so, we might say that the tribal organism was now again adapted to its environment, whereas before its shift of base it was injured by its narrow environment and by the fleet's continued overgrowth. But would this adaptation last? It would if new stresses and needs were not developed. If they were we should again see growth, perhaps on the old lines, perhaps on a land military line. There is a distinct difference between the two orders of variation, though they tend to merge one into another, one being Lamarckian, the other due to inertia. Any student with some knowledge of history will see that changes such as those suggested have continually occurred and are now occurring in national organisms. And very little insight into the processes concerned will show that many disturbances of social evolution are in a great sense morbid; they are variational disturbances following the line of breakdown and repair. It should be noted here, if it were not done elsewhere, that Cuènot in *La Genèse des Espèces Animales* has developed this view of the organism "making use of" changes which force it into a new environment. It was only lately that I learnt that he had done so.

Function in Evolutionary History.—And here it seems advisable to say that though no one, so far as I am aware, has before this suggested that there is a real, even if slight, element of the pathological in heritable function changes, function in the pre-Darwinian era was held by some of the greatest figures in zoology to induce heritable change. As

I cannot pretend here to give even the slightest sketch of the history of evolution, to mention the name of Cuvier will suffice. Very notable work done after the publication of *The Origin of Species* reinforced these ideas of functional heredity. Gegenbaur certainly believed that somatic variations could be transmitted, though he held the view I have constantly emphasized that the organism was naturally highly conservative. But in spite of that it was capable of such changes as Lamarck suggested. Even if he accepted Darwin's theory of variations and their accidental accretion he combined this with an admission of the plastic value of function. But however much Darwin relied on Natural Selection he never ignored that wholly, since it is implied in use and disuse. It may be added here that another German investigator, Kölliker (1864), seems, as Mendel was then not exhumed from his grave, to have been the real inventor of mutations, for his saltational variations were clearly modern mutations. We seldom see Köllicker credited with this by Neo-Mendelians, for what it may be worth.

The Giraffe.—If such a social illustration as that given above fails to produce conviction the fact that forced environmental changes may at last result in a line of overgrowth is well seen in the giraffe, a case of continuous variation in which Lamarck's explanation is often scoffed at. But supposing he was right in attributing the earlier changes purely to forced function, there is no difficulty in conceiving that it was those which started the later overgrowth, an overgrowth which becomes obvious when we remember that the giraffe cannot now when adult drink from a shallow pool without a wide straddle of its forefeet. Clearly a further overgrowth on these lines would result in the death of the species, or in inherited bone and muscle changes all on a continuous line of continuous function, which could not logically be attributed to small advantageous variation or to mutations. If the overgrowth

still continued it would, however, in the end lead to extinction. Breakdown may not be repaired for ever unless the environment remains, or becomes, favourable to the acquisition of sufficient energy to ensure adequate repair, which palæontology shows to be unlikely, to say the least of it. This view may be reinforced by new work in palæopathology such as I understand to have been lately done by an American zoologist.

Roux on Functional Stages.—Without claiming too much it may be said that to raise failure and repair to the rank of factors in evolution seems so new a notion that any general assent to such a hypothesis can scarcely be looked for, even if it is presented without additions likely to excite prejudice. Nevertheless, the more the views expressed are considered the more evident the underlying trend becomes. That the changes indicated have occurred is of course obvious on the plainest anatomical and embryological evidence. So far as our present knowledge and the theories based upon it go we have to choose the machinery of these changes from the armoury of Darwin himself, or from his scientific descendants who have deprived him of use and disuse and have substituted the gene and mutation theory for his spontaneous variations, or from a modern modified Lamarckism which regards function as a heritable director of growth and metabolism. It is true that a revolt from the rigid Weismannism which so long stultified research has resulted in a strong movement among Neo-Darwinians towards function as a morphallactic factor. They are, however, stayed by theory from admitting that what acts now has always acted and therefore function must have created the earlier structure which determines present functions. For thus "genes" would be nothing but raw material with all its potentialities for function to work on. Function would make them and put into the cell as evolution proceeded the necessary tools it picks up on the way. It was J. T. Cunningham to whom we owe

anything like a theory of descent through hormonic action and it ought, at the present time, to be obvious that all we know of the endocrines has thrown evolution, or rather its mechanism, into the melting pot. To Roux we owe the fruitful expression " the struggle of the parts." When extending his obvious meaning by using the phrase " Hostile Symbiosis " to express the underlying partially suppressed hostility of autonomous, or semi-autonomous, tissues and organs, I was not then acquainted with all Roux's conclusions. Perhaps he did not see this particular one was an organon for dealing with every kind of phenomenon capable of being classed as evolution, such as sociological and even cerebral mentation phenomena. But all he said of first and second stages in evolutionary variation fits in with what has been remarked of past function or what I have called " sunk function." An animal organism is as much the creature of its past history and historic environment as any society, and all historians know that the legal and moral skeleton of a country has its origin in variations which, as the race functioned and tried them out, gradually hardened and were perpetuated, while the residual plasticity permitted further heritable change. In a comparatively light and casual book such as this none can expect a complete review of functional morphology such as that attempted by Roux. Still, some additional suggestions may be made. There is no real need to distinguish between the stages where function rules and those where it does not rule. For life *is* function. Even in the smallest fragment of what we call living matter there are recurrent and rhythmic molecular reactions such as those seen in colloids approximating in alternation to sols and gels, but never being completely one or the other. We are therefore justified in inferring that molecular functions in increasing aggregations of living matter gave rise to structure and are always doing so. It is true that function tends to fail with age, for then bound energy overweighs free energy. But in

young organisms energy is for ever building or repairing what has been built. That the scheme or worked out plan of the body becomes up to a point sunk into the germ is what might be expected from infinite repetition, but I esteem it an error to say that this set working out of germ potentialities is a non-functional process. We have no right to assume that any set organic scheme, discovered by trial and error, can be carried out without function. The difference is that there is more certain functioning in the early construction than in the later. Each stage as it functions incites less certain function and functional results. When the adult organism functions it is at a stage liable to all sorts of errors. So, differing here from Roux, I should say that there are not two or three stages but an almost infinite number of stages, each distinguished from each other on the ascending or more complex scale by a lessening certainty of correct function. Thus it may be argued that form and structure of all kinds arose in function and that structure is really repeated by definite embryonic function not yet properly known, though when these structures are completing they act in the later less certain functions as determinants, directors and controllers. If at any stage they go very wrong false structure will result. Any slight error should lead to microscopic, not macroscopic, failures revealed only by faulty functioning. Any difficulty in this view will be due to a failure to recognize cellular and embryonic working as functional, or to the belief that perfect " ascertained " functioning is not functioning at all. I can see no reason why Roux should not have subscribed to such a view of evolutionary morphology, and I must leave it at that, though I am certain that such a reduction of life and structure and heredity to a purely mechanico-physico-functional basis will arouse the contempt of those to whom such words are but a red rag.

Breakdown and Repair.—In *Evolution and Malignancy* it was sought to show in what way the " struggle of the parts "

could be applied to the endocrine struggle for the individual dominance of each ductless gland, which is continuously met, if the organism or species is to survive, by the reaction of one endocrine against the other. This naturally led to a great extension of the theory of immunity, which, as I venture to think, was shown to be an almost unrecognized evolutionary factor. It is now clear that these tentative conclusions can take their place in some fresher view of Lamarckism, but cannot without distortion fit in with the more orthodox theories.

Theory, however, may be for the moment put aside when we consider the evolutionary rise and increase of the cardiac and gastric musculature which was treated in *Warfare in the Human Body*. We have there facts and not theories. Can we believe that these increases were due to accidental variations of a minute kind or to large mutations? The variations were in each generation almost certainly minute. Would Darwin have said they were clearly due to accidental increment? If so would the advocates of orthogenesis have flatly denied it? Here we have a continued and continuously more useful and adequate structure, although it is still liable to breakdowns. If, as deniers of evolutionary mysticism flowering in preformation or predestination, we are bound to put aside notions so obviously founded on ancient prejudices, we can only fall back on continuous function unless we avoid the Scylla of orthogenesis to fall into the Charybdis of the gene and mutant, both of which now show signs of decay at the root even as they flourish in the flower. Till we are told how genes come into existence they are but a form of disguised preformation.

I do not see how it is possible to deny that stress, breakdown and repair occur for ever in any organism from some primitive protoblast to a world society. This means, put quite plainly, that disadvantageous variations can be, and clearly are, factors in evolution. Without some scheme of foreseeing, or latency, we cannot assert that the healed

reaction-results which finally are, for a time at least, advantageous, are only to be considered. We must consider how they began. And, if it can be shown that even a mended and repaired organic disaster remains, if not actually a disaster, at least a permanent danger and disability, the whole theory of unmitigated survival value in advantageous variations must be put out of court. I would not again labour the instance of the forced descent of the testes if the question were not crucial. But I cannot refrain from asking for a clear and convincing explanation on orthodox lines of that external evolutionary hernia, the testicular sac. The reply that declares it sufficient to know that in many of the mammalia the testes do not grow properly or function when still retained in the abdomen, has no force whatever when we note that in the mammalian testiconda they function as they should, and that in the boar, though extruded from the bodily cavity they are at least protected by a thick normal skin, in no way partaking of the obviously later character of the human scrotum. It may be said, I hope without offence, that the indoor zoologist is at some disadvantage in considering these matters as compared with the outside observer or the field naturalist. And yet many systematists or laboratory workers do at least keep dogs. Have they never recognized that their wise companion is wiser than they might think, and that he knows in the clearest way that his great weakness lies in the possession of external testes ? I have heard it stated by unmitigated holders of the advantage theory that the use of the dog's tail is to help him to turn ! Assuredly the human mind works very remarkably. A little observation would show that the tail turns, or appears to turn, when the dog does, because it " lags." Here is a case of tail " hysteresis." Let them observe that the " accidental " possession of so many tail vertebræ gives the dog something to tuck in so that his pursuer may not gain a decisive advantage over him by laying hold of the

unprotected testes. So it is with the horse, which also knows well where the danger-point is. Selous remarks that the prowling hyena sneaks up quietly to a grazing jackass and makes his first hold on the part his knowledge of his job tells him is fatal. It might be suggested that the early death of many young male animals is due to this fault in construction. This danger is so well known among men that in parts of North America the expression to get a man "where the hair is short," and thus to render him utterly helpless, is an expression for grasping him by the testicular sac.

We need not insist on the frequency of accidents to the testes, though any surgeon can bear witness to them. Every man, whatever his mode of life, must frequently have recognized the dangers to which they are continually exposed, even if they have not been actually damaged. The late evolutionary stage of the repaired hernial sac we call the scrotum is shown by its poor musculature and also as it seems to me by its liability to lymphatic stasis and chronic eczema. But surely there is no need to labour the point. It would take a fanatic to believe the position of the testicles advantageous, and the fact that the human testes cannot now flourish internally is but another proof that organs can during vast periods of time undergo changes due to descent with modifications. I decline to discuss "mutants" as the basis of any reasonable explanation.

Evolutionary Operations.—That the process by which this extrusion took place must have been gradual is quite clear. The embryological and anatomical evidence declares it, unless the Mutationists are ready to tell us that the ablation of peritoneal tissue now forming the *tunica vaginalis* was in fact a mutant. Keith has very often pointed out processes which he describes as the healing of evolutionary wounds. Following his courageous example I declare this last testicular feature mentioned as "an evolutionary operation."

Those who are interested in surgery are aware of operations done in two or even more stages. Here we have one accomplished during numberless generations, and in the embryo we see how the stages succeeded each other, sometimes by slow and probably minute alterations in position, and once at least by a change that cannot be called minute, since at the point of final extrusion the testes had to be in or out. The slow preparation, even now imperfect, of the scrotal sac, is very obvious. I suggest, since evolution is always in progress, following on function, that there is some possibility of distant generations being the happier possessors of a more secure depositary for the means of carrying on the human race. The scrotum may yet be "healed" and become a powerful protective bag.

I am wholly unable to see in what way any of the accepted doctrines can help us to understand how this striking drama in so many scenes and acts came to be staged on the evolutionary theatre. We have necessarily to fall back on the successive inheritance of somatic changes brought about by a complex of environmental causes, many of which we cannot yet guess. But since with our surgical knowledge of hernias we are able to assign reasons for them in those whose abdominal weak spots are not proof to stresses, either accidental or long-continued, we can postulate a weak spot selected by the testes under abnormal stress. It is not affirmed that the upright posture alone has been the main cause of this evolutionary rupture. In face of the quadrupeds in which it has happened this would be absurd. But may it not be suggested that it has certainly been a factor in the Primates, and that abnormal activity in the dog and horse and other animals which jump and rear has brought about a like result in them was plainly stated by W. Woodland in 1903. His arguments were clearly Lamarckian, though he did not mention that zoologist. It may be remarked that the elephant and the whale, the largest living testiconda, are rarely, if ever, subjected to similar strains.

Bearing in mind the embryological phenomena, are we not forced to believe that it took an immense time for the actual peritoneal operation to be completed? But, however slowly or quickly it occurred, we observe a perfect succession of stages, not explicable, without more stress than most imaginations can bear, by any unconnected Darwinian variations, or by genetic mutations. Such a right line, ending not in advantage, but in a decided disadvantage, can only be regarded as a Lamarckian succession of hereditary states, beginning with the postulated weak spot, and rendered easier and easier generation by generation as the passage opened, owing to a changed set of functions and an altered blood and lymph supply, at a place already less strong than it might have been.

Pathological Variation.—It is clear that this employment of pathological conditions in explanation need not imply continuous severe discomfort during the evolutionary process. Many important somatic disorders occur without pain. At certain stages, however, we must infer that changes led, not to repair, either in the parents or offspring, but to real disaster and death, or possibly to sterility. Here then in a Lamarckian process we can find Natural Selection actively at work, at its proper task, which assuredly is not to provide the raw material of variation. It will some day be the task of a biologist adequately equipped with pathological and even medical knowledge to discover and endeavour to elucidate the actual factors now at work in the evolution of the human race, which can be termed pathological as leading to the disorder implied in forced variation. After what was said above it need not now be too much insisted on that his greatest aid to diagnosis may be found in the study of social evolutionary process going on in the world about him.

Mechanism of Heredity.—We are, it must be owned, not yet in a position to state the whole mechanism, on any principles, of such changes as those noted, nor can we say

with absolute certainty how they are embryologically prepared for. But since we begin to know, chiefly from workers in very plastic organisms, how it is that a simple experimental change may stimulate or prevent some expected change, we need not despair of a final solution of this particular problem. When we come to it, many other problems will be solved. We shall, for instance, know why and how it is that teeth, created from epithelium by continuous stresses, gradually sink back into earlier stages of growth than those in which their protophases were formed and are thus given the aspect which many can only contemplate as being in some way foreseen. That this is a hopeless denial of explanation seems clear, but I find myself able to comprehend it when I observe many working on evolution with but one or two of the many tools provided by the current theories in all the biological sciences. It is remarkable how little many gifted men are able to indulge fruitfully in criticism. They are dominated by a modern form of fatalism.

Lamarck and C. M. Child.—In this chapter I have not attempted anything like a complete review of the arguments in favour of Lamarckism set forth by those biologists who have followed him. This is not because they are weak. But they have been so often repeated that it seemed better to add others if they could be found. Those adduced may, however, be somewhat strengthened by what has been said by C. M. Child (1924), who, without classing himself in any way as Lamarckian, has been a great pioneer in investigation and ways of thinking which lead to conclusions not alien from those of that zoologist. My conception of an organism as an evolving dynamic partially closed system continuously influenced by the environment and changing from states of relative health or order to states of relative disease or disorder receives much confirmation from his work. It is true that he does not use the vocabulary of the pathologist, since behaviour and behaviour patterns can obviously be

treated as he treats them, but I cannot conceive his denying that the changes both in behaviour and protoplasmic pattern may be considered in the light of disaster and repair. For he says (p. 217), " Life apparently consists in a succession of disturbances and alterations by external factors and in the reactions to them, and there is abundant reason to believe that in the absence of such disturbances and alterations life would soon cease and static equilibrium would supervene." And again (p. 222): " Actually, however, it becomes now more and more evident that life is largely, if not wholly, made up of accidents." I do not pretend to determine here if Child has any definite leaning to any form of Lamarckism. It would be presumption to do so, but though he rarely if ever refers in this book to Lamarck his language in it and in other work rarely contradicts the possibility. For he, at least, never divides the organism from the environment, and thus avoids even the shadow of preformationism which still lingers about the early stages of development in the work of the orthodox English school. If nothing is found in *The Physiological Foundation of Behaviour* which contradicts ascertained facts and much that confirms speculations based largely on pathological findings brought into some relation with other sciences, this helps to show that the method adopted here is not unsound. Such a method does not imply " synthetic judgments a priori," which may be left to orthodoxy with some confidence that it will continue to use them. Cross deductions from the conclusions of all the definite sciences dealing with life, which confirm each other, afford a legitimate method of reasoning which might well be used more frequently. By it we learn to recognize those obscure likenesses which, when seen, bring real illumination in the most obscure problems. By it the organism is shown to be a partially closed system, always with something about it analogous to a semi-permeable membrane, which subjects it to the determining factors of the environment, and by it we can learn, or at

least very strongly suspect, that the rhythmic alterations between comparative rest and comparative activity, between static and dynamic states, are comparable with, and even analogous to, the colloidal rhythm of sols and gels. Again I may quote Child: "Evolution is a process of the standardization of the potentialities of behaviour, of regulation, and of its mechanisms, and, through this means, of the environmental conditions to which the individual is likely to be subjected." And when he adds, a little later, that "the reaction to environment as equilibration may become a factor in evolution," we may well agree and even go further and say that these reactions are a main method of adaptation, and that his words are strictly in consonance with a reasonable modern interpretation of Lamarck. The work done by Child has been and will be a landmark in modern biology. It will have an increasing effect. If some of it seems at times somewhat obscure that is due rather to its freshness of outlook than to any failure of expression. The new is always difficult when it necessitates any change in a student's settled orientation of thought, while it is easy and very pleasant to be agreed with on one's own lines.

CHAPTER FIVE

A STUDY IN GENETICS

Genetics and the Tadpole—Rationalization in Science—Mutations and Function—The Ghost of Weismann—Hormones and Catalysts—True Epigenesis—"Natural" Development—Left-Side Dominance—The Experimental Environment—Psychism and Biology—Predetermination—Regulation—Spemann's "Organizator"—Self-Differentiation—Function—The Passage to Physics—Bone—The Osteoblast—"Inherent Properties"—Doubts about Lamarck—Chemical Skeleton—Acromegaly—Hair and the Gonads—Uterine Growth—Accidents to the Ovum—The Alpine Dandelion—Steatopagy—The Glands of Swammerdam.

Genetics and the Tadpole.—As the last chapter was devoted to the pious task of attempting to rescue Lamarck from his enemies and also, in a measure, from his very friends who do not recognize the great time factor on which he laid such stress, the chapter following may take up an even more difficult task, that of rescuing Darwin himself and possibly Mendel as well. For Darwin when stripped of use and disuse is in truth no longer Darwin, and it may well be doubted whether the Abbé would have agreed with modern geneticists whose passion for mitotic patterns leads them to ignore cytoplasm and the cell "as a whole" in its environment. It is true that they are not wholly unanimous: there are differences of opinion sometimes openly confessed, sometimes almost cryptic. Occasionally they seem to have dreadful doubts as to strict doctrine of the gene. The cynical critic may even imagine some of them waking in the night and wondering with alarm whether that magic deity of heredity is really as omnipotent as they thought it. All, indeed, are not strict and strait-laced Mendelians:

some move towards an inexplicable orthogenesis : some lean towards latency, and others, keen on bench work which demonstrates the mighty powers of environment and the propinquity and grouping of tissues, really labour for Lamarck. To consider all these sub-sects with their doctrines developed in innumerable books and scattered in vast unindexed profusion over a multitude of scientific journals would take a lifetime and a volume beyond the power of man to write or of men to read. But since what has been said directly of Lamarck was light or merely suggestive it seemed to me that instead of attempting the impossible it might be as well to deal just as lightly with one prominent upholder of Neo-Darwinian-Mendelian genetics. Much of this book was written when I was thousands of miles from my own small library and the great technical libraries to which students may gain access, but as I happened to have with me a book by one biologist who had attempted to interest the indifferent world in his science I determined to concentrate for the time on this particular work. For though its treatment was necessarily somewhat dogmatic, it seemed here and there as if the writer was not so certain as he seemed. It might then be possible to deal with his conclusions and his doubts together, and, while acknowledging as far as possible the merits of genetic work, to show that genetics could not solve everything. For rightly or wrongly, rashly or with proper confidence, it seemed to me that even geneticists might some day so vary as to adopt a more generous view of a reasonable eclecticism. The book referred to, my sole resource in a bookless wilderness, was *Essays in Popular Science* by Professor Julian Huxley. Perhaps this accident was fortunate. Certainly in other cases it has seemed best, when dealing with very wide but strictly connected subjects, to concentrate upon a single generalized statement rather than to wander over the whole field. In this chapter I shall therefore concentrate upon the Huxleian *Tadpole* as a text.

Rationalization in Science.—If this biologist is classed amongst those who often work for Lamarck without knowing it, it will do him no injustice. His love of truth, his native ardour for research, are such that he would no doubt renounce his biological Church if he found good reason to do so. This is not always the case. The passion for rationalizing mere prejudice is deeply implanted in mankind and even professors are men. The most religious people are said to have awful crises of cold and dreadful doubt in which the sky empties and they stand alone without even "a friend behind phenomena." But when the morning comes they indue confidence with their clothes. It must be so even with some votaries of orthodox science. They may in one moment see all the reasons for a faith opposing their own while their best arguments seem leaves in the wind. Such may "rationalize" in sheer desperation. For some cannot change their ritual without utter disaster.

Mutations and Function.—Yet though so much might be said even about this witness for orthodoxy, he is, most undoubtedly, a good witness to what he thinks the truth. He believes in Mendelism and carries his faith in the Gene to the point of weighing it, perhaps in company with Morgan and certainly in company with J. B. S. Haldane. He tells us that its weight is an infinitely small, but yet measurable, fraction of the weight of the Universe as calculated by Einstein. This is a delightful speculation which may well charm the curious. He believes in mutations and thinks they take the place of the pure early Darwinian minute advantageous variation. There is no obvious passage in which he shows any belief that a gene or factor can be acquired and here he seems to agree with Punnett, who once said they cannot be acquired but can, in some miraculous way, be lost. Huxley affirms his confidence in function as the present shaper of what is given in the chromosomes but denies that what works now worked of old. If all this does not mark him as orthodox we may well ask what

orthodoxy is, especially when we find him definitely putting aside Lamarck, though in one place he seems for one sad instant to waver for just one moment (p. 289).

The Ghost of Weismann.—And yet, has not this orthodox witness parted with what was his orthodoxy in 1921? It must be remembered that now the world has given up pure Weismannism. The prophet has fled from Mecca and no Medina receives him. So far has this desertion of a once sacred cause been carried that I have often been rebuked for wasting time in slaying the slain. But as we have seen and shall see again, it is a question as to whether the ghost, or duppy, of Weismann is not still a power. How great he was in 1921 J. S. Huxley himself shall testify. In a review in *The Nation*, Feb. 26, 1921, he wrote, apropos of somatic modifications, the very remarkable phrase: " the particles in the germ which represent muscle." Here we have ids and the like, naked and unashamed. Yet would this worker say as much now? Probably he would not. We should get something weighable and measurable called a gene, or two genes, or X genes. They might be called factors, for that word sounds far more solid and real than ids or genes. Still our professor has in fact moved far since 1921 and we may hope to see him move further.

Hormones and Catalysts.—It is well known that J. T. Cunningham was the first who plainly stated that the phenomena of heredity were due to hormones. Since then we have learnt so much about the ductless glands as to give rise to the branch of physiology called endocrinology. Here let us say again what Claud Bernard said long ago. There are not properly many sciences. There is only Science. Divisions are for our convenience, and it is a pity that with those conveniences we have such grave inconveniences that an expert on one bough of the tree mistakes it for the tree itself. Is it such a failure that makes some biologists recognize the immense value of the endocrines in growth and form and yet be blind to the fact that a new

secretion, *i.e.*, a highly complex chemical body, had to come into existence before it could do anything ? We have not yet heard of genes for thyroid secretion, or for the two pituitary and adrenal products. And are there genes for trypsin, or rather trypsinogen and the like ? Huxley, quite forgetting his hatred of the environment, quotes the fact that in the "goitre belt of the United States it has been shown that the overgrowth of the thyroid is due to the overgrowth of the gland endeavouring to compensate, by over-production, the absence of iodine in food and water." He goes on to say that the water iodine and thyroid growth rate determine the course of a tadpole's metamorphosis. Is it really wrong to say he is working for Lamarck ? For surely there is no gene for iodine itself, nor is it a gene. True, it will be asked what has all this to do with descent with somatic modifications. It has this to do with it. Iodine, when picked up and used, was and must have been transmitted to the egg and egg yolks and albumen, so that what modified the parent modified the offspring. Moreover, as many endocrinologists believe, hypo-thyroidism can be acquired and inherited. All that has been learnt of late years shows how greatly the form and function of the organism depend on food factors. We may say as regards a whole race that the amount of iodine used is an environmental modifying agent, certainly not to be attributed to any gene or chromosomatic factor until it was put there by environmental accident. Without it, that is if the race were deprived or even partially deprived of it, we should get a modified race and once again this would not be due to a lost gene but to a very definite external factor leading to iodine loss. The same argument applies to iron when it was first used by the ancestors of the mammalians. Then again it applies to the crustacean-like animal which first used copper as a catalyst, as well as to those birds in which that metal takes a part in the pigmentation of their feathers. That these modifications are inherited is obvious, though

iron and copper are continuously reinforced during life. They help to determine form, size, growth and colour, and though, if these animals were taken out of the environment, such factors would gradually be dissipated with concomitant growth failures, they might last for generations. And it may be said here, even if it is a repetition, that the Lamarckian always postulates for the continued inheritance of any modification a similar environment or continuous function.

True Epigenesis.—Perhaps we may next note that Huxley testifies to his belief in epigenesis, by which he means a real increase of complexity and real differentiation occurring during the course of development. How far, and how, this is compatible with the gene theory we should learn definitely. And we should have to ask whether his view of epigenesis fits in with that of those workers who deny that a Neo-Mendelian can be a true epigeneticist at all. It must be owned that this question of epigenesis comes near to bringing up the verbal squabbles which are the natural subject-matter of metaphysics. For, when considering it, Huxley at once takes up the question of the hypothetic genes which, he says, in the germ " are disposed in order along the bodies we know as chromosomes, and, since the chromosomes are self-reproducing their sum really represents one vast chemical unit, whose parts—the separate genes—occupy a constant position in the whole, just as do the atoms of a simple molecule." Not a whole book could dispose of the questions raised and the assumptions made in these few lines, but we may well ask what bio-chemists, who have turned down Ehrlich's side-chain theory for the obvious reason that no such molecules as he postulates could exist, would have to say to this vast chemical unit. And if we grant that such a unit can exist, a unit in which every determining part, or gene, has its place, how are we to distinguish this kind of epigenesis from a disguised preformationism? It is true that Huxley, for all his belief in this all too clear definition, immediately departs from it as being of any value when he

admits we know nothing whatever of the way genes influence character. And then being cheerfully rid of this genetic white elephant, he goes on with his proper work and does it amazingly well by showing that in every experiment environmental "accidents" are the determining factors of the developmental path, which, however, naturally becomes more settled and less liable to divagations as each stage is reached and passed in safety. It is on such grounds that it was said that many apparently confirmed Neo-Darwinians and Mendelians were really working for Lamarck without knowing it. Such innocence in men of original force is by no means so rare as might be imagined. For example, we may speak of the grey crescent in the fertilized frog's egg which determined its future plane of symmetry. This is caused by the entry of the sperm. "Thus one of the most fundamental attributes of the future frog is not pre-determined in the egg at all, but is fixed by the accident of the sperm's point of entry." He goes on to add that it might be fairer to say that the egg has the potentiality of becoming bilateral about an infinity of axes and that the sperm only determines which shall be chosen. There is no essential difference in these statements. The genes have nothing to do with the plane of symmetry of the frog. So far the Gene is discharged without a stain on its character. Those, who were all-powerful, in one great case can do nothing.

"*Natural Development.*"—The blind ardour with which effective research is pursued is so intense in those gifted with the necessary faculties that we need not be surprised to find that no conclusions as to the power of gravitation, say, in altering the relation of yolk and sperm, are looked upon by Huxley as relevant to an environmental effect which is not germinal and yet may determine relative grades and states of development. I do not note, and, indeed, have never noted anywhere, the very obvious conclusion that monstrous, or what we naturally call monstrous, growths, might, unless they are excessive and really destructive, be

looked on as possible natural paths of development. What for instance would the human being have been like if part of the embryo were not "amputated" during placental growth? Would it have been a "monster" or are we not all "monsters" because we lack part of our real germinal inheritance and bear the umbilical evolutionary scar to bear witness to the fact? Jenkinson centrifuged fertilized frog's eggs before segmentation and dislocated the egg's constituents according to their specific gravity. There were different grades of degeneration or non-growth following the process. If such a dislocation had resulted from "natural causes" we should have had a different batrachian, if the change had not been too gross. We again come back to factors of position, nutrition and environment that are not genetic or germinal.

Left-side Dominance.—To what an extent pure hypothesis, for now I must refuse to speak of the Gene "theory," may dominate the most clear-sighted is well seen in Huxley's remarks on the common left-side dominance, found in embryos, which apparently the experiments of Spemann demonstrated. Huxley says that when this asymmetry is exaggerated in any way "it may in some way pervade the body and induce secondary changes." Let us note the very modest "in some way" and then note that he considers such dominance the cause of the universal atrophy of the right ovaries of birds. "The left side must therefore be more dominant than usual." How can we fail to be astonished to hear nothing of genes? And with what pleasure we greet our old friend the word "dominant" which we feel suggests, as opposed to it, something like "recessive." Who would be surprised if he heard that an atrophied right ovary was in other animals a "recessive characteristic" and that this recessive became dominant only in birds? However, this is not insisted on, and "who would not smile" among the orthodox if it were suggested that an ovary, atrophied on account of some

internal or external environmental influence, was actually carried over hormonically in a particular species, and that it might have become hereditary, if for obscure reasons yet beyond us, the bird did better or at least as well with one properly developed and fed ovary than with two? Certainly there is guesswork here, but we are guessing about known factors, not about "imaginary numbers" in a pseudo-mathematical game.

The Experimental Environment.—How cheering it is to turn from these speculations and read an admirable summary of work, done by those who were untroubled by them, on the actual processes of growth in the experimental laboratory. This part may be read with pleasure and much profit by many biologists who are far behindhand in this great and almost new branch of knowledge. I shall not, and indeed cannot, summarize so good a summary. It must here suffice to say that while there is in the work, read casually, much to disturb the scientific pietist, there is also nothing that the environmentalist would not admit. Perhaps the odd insistence on the varying constitutional effects of forced experimental changes might make the very suspicious orthodox reader wonder what he was coming to, since some such experiments are always being tried by "Nature," the greatest experimenter of all. Yet nothing could, on the whole, be clearer than the demonstration that one state induces another, that one part influences and is influenced by others, and that early and very powerful growth centres can be found and displaced with remarkable results. Thus the dorsal lip, the powerful "differentiator," can be removed and grafted elsewhere, and then the medullary plate is induced about it in a totally abnormal position. True that Lewis, long before Spemann worked this out, showed that the optic disc when replanted under epidermis formed there a rudimentary lens and thus opened up avenues for joining all work on known hormones with these obscure quasi-hormonic tissue

messages, and true epigenetic growth. And so we go on and presently come to Child, who may in some sense be said to be the parent of all these new investigators, who discovered "axial gradients," or graduated differences in the metabolism of different parts of an organism. Some knowledge of these must be taken for granted, and here we can but say that if their rates now seem constant they yet had to be established, and what has been established can be changed. And that this is so Child showed (1915), for various degrees of poisoning can change them. So, when in thinking this over we reflect how greatly the animal body is the subject of unavoidable intoxications and is therefore, as we must infer, partly their creature as it grew up with its present comparative immunity, it is not possible to avoid imagining that those axial gradients, which are assuredly not genetic but epigenetic, may alter even to-day, and even in us, for any number of environmental and nutritional reasons.

"Psychism" and Biology.—Here I shall not speak at length of Huxley's use of the word "mind" in such a phrase as "that part of the brain *associated* with mind." I have italicized one word here, as it reflexly suggests the dim and distant source of too many orthodox dogmata. Elsewhere I shall attempt to show what can assuredly be shown, that the use of such a word as "mind," unless it is avowedly no more than a shorthand word for reflex cerebral and nervous workings, betrays, if not vitalism, something not too distantly related to it, something which may be called "psychism." It is true that Huxley admits that the ductless glands have much to do with "the psychic side of life." All these phrases are to be distrusted. They cannot be used by any resolute determinist. There is some religion always at the back of them, and science has nothing whatever to do with religion, if it does not study it as human behaviour. Whenever we find something like "unpacking," or orthogenesis, or latency, or any-

thing like the veriest shade or shadow of an entelechy, we are justified in believing that the pure equation of science is not to be found. Whatever the protestations of the followers of the Gene I shall class them with these others, even if they weigh and measure their godlet of the chromosomes.

Predetermination.—How refreshing it is to desert these prehistoric dens and caves, in which the gnawed white bones of dead faiths and fears and fancies still move so many, and get back to the clear bright air of bio-chemistry, in which we may forget all about mind and soul and spirit, for where " mind " is all these other ghosts may be looked for, and behold things working in a series of chemical reactions, each one bearing on another and two influencing a third, and all reacting on each other without vitalism in any test tube of the lot. It is true that in Huxley's admirable disquisitions on what he calls " chemo-differentiation " he frequently uses the word " predetermined." But he quotes Spemann, and we shall not suspect Spemann when he uses the word with regard to the pre-determinism of the optic stalk, the pigment or tapetal layer and the retina in the medullary plate before it sinks in and forms a tube, long before we can discern the first rudiment of the eye. To chemic predetermination in this sense no more objection need be made than we are likely to make to the simple statement that the formed human embryo becomes a human being, or that the adult's total characteristics are at a given state " predetermined " within close limits. But we must be allowed to presume on pathological evidence that at any stage the process may be altered or interfered with. The predetermination is therefore not absolute but relative to the internal and external environment. We have here true chemic, or bio-chemic, epigenetic determination and differentiation, and, granted the first series of chemical equations, we can infer that they " naturally become " the next set, unless

a factor is interpolated or subtracted. Thus in truth we have no use or need for the word "predetermined." It is merely descriptive of what occurs normally and we have, if we care to do so, just as much right to say that the dorsal lip, or the medullary plate, is predetermined in the normal arrangements of the electronic rings of atoms in protoplasm and in the factors we call catalysts. The word can be done without and we may sometimes infer that it is merely retained to comfort those who feel cold without some "predetermination" at their back.

Regulation.—Yet though we may perhaps legitimately look to see if Huxley seems to trip in these important verbal matters, it must be owned that he turns down very definitely those who look upon "regulation" as so essentially a vital affair as to demand a Drieschian entelechy, or, as Dr. Haldane appears to think, some system or new category in which to class living matter. Huxley speaks of these notions as regards what he calls the regulating and non-regulating periods of embryonic life in some organisms. The pre-gastrular phase implies continuous regulation even when the embryo is cut, so that bisection produces two animals. In the post-gastrular stage there is no such complete regulation. The parts reproduce only what they would have produced if the embryo had been left whole. I leave it to Haldane and his followers to determine if these facts are of any real importance in theory. I myself should say that at the close of the gastrular era regulation has so far become settled and local as to lose any great possibility of change. May we not suspect words are at the bottom of any difficulty?

Spemann's "Organizator."—And here "I regret to say" that Spemann whom, on certain information, I acquitted of genes in the matter of predetermination, falls a victim to his passions and calls the dorsal lip centre an "organizator," which Huxley, following a bad example, merely

turns into "differentiator." On other occasions it has been necessary to speak somewhat disrespectfully of the vocabulary beloved by bacteriologists, who, upon the discovery that in certain conditions a very complex colloidal reaction takes place, state, probably for their own peace of mind, that this reaction is caused by a word ending in *in*. Is there any reason why we should not class "organizator" and "differentiator" with such words as opsonin, agglutinin, and the like? For all that Spemann and Huxley mean, and can mean in the present state of knowledge, is that when one development takes another follows. That is merely to say that a curiously complex state is followed epigenetically by one very often still more complex. No real objection can be made if this is thoroughly understood, but the presence of the word "differentiator" is all the same objectionable, as it tends to produce, perhaps by some cerebral colloidal reaction following another, a belief in the differentiator as an entity having peculiar powers, for let us note that Huxley, with enormous courage, says "the differentiator is a single agent." It affords me some gratification to link it up with that other fantastic quasi-deity, the "censor" in the latest psychology. At present we should stick to purely epigenetic words.

Self-Differentiation.—The sections in the Tadpole paper on the self-differentiation of organs, or their differentiations as dependent on other structures, will be interesting to biologists, or not interesting, according to their ways of looking upon growth generally. That certain parts of the animal's body should be self-differentiating may be true up to a point but in fact it means little to say so. In any case, in the body or *in vitro* the part in question has to be nourished and supplied with salines and other catalysts and hormones which do not belong to it as a separate part. No one can yet state the factors or effectors, whether of a hormonic or catalytic type, which are thus

conveyed. To say that other parts can be shown to be dependent differentiators is merely to state that they take a later place in chemo-differentiation and require as effectors of growth still more complex and later stimuli. They occupy a later stage in somatic epigenesis and we learn nothing, or exceedingly little, by putting the facts into more complex phraseology. At any rate we get nothing like the explanation or even suggestion of explanation we were more or less led to look for. I rather gather that Huxley himself is secretly almost of this opinion, since he states the one new theory of importance, learned in a discussion on lens formation, is that " contact is necessary for the passage of the differentiating stimulus." Let us put this as a child might and see if it is new or important if we lay it down as a grand principle that some organs need a more concentrated " hormone " than others. So what will irritate Jones will not irritate Johnson.

Function.—Following our leader we must now get to Function and a very interesting subject it is, so much so indeed that most of the coming biological battles will probably be fought out on this ground. Already smoke and poison gas float over it, and, as might easily be guessed, even now a kind of genetic mysticism seeks a footing there. The first text is founded on Roux's perfectly accurate statement that in organisms there is a functional and pre-functional or embryonic state. I say perfectly accurate but it might be better to say perfectly accurate as far as it goes, though as a matter of fact Roux believed pre-functional states to have been functional in phylogeny. The statement goes no further than a very little way as Huxley quotes it. It is roughly true so interpreted but not quite true, for large portions of an embryo are clearly always functioning and there is every reason to suppose that all parts do so. To function is to act, to do work, to do a job under stimulus in definite conditions. I shall be interested to hear of any embryonic state or stage in which

very definite functions are not being fulfilled, when jobs are not undertaken and carried out "with accuracy and despatch." We are scarcely in the position to underrate the powers of protoplasm, since all we do is done by protoplasm. We might still follow Hunter with advantage when his keen brain saw perfectly clearly that the tissues he watched in bone and elsewhere possessed a kind of "consciousness," the consciousness which is infinitely more in its beautiful certainty than those "higher" or more complex cerebral reactions of which we are overproud. Long ago it came to many with conviction that there was, and could be, no break in the chain between our fumbling doubtful cells and those still more populous units that make up the great social cell-city, and between those and the atoms and electrons with which we struggle now to do our work. So how and when and where shall we distinguish between functional and non-functional states, unless we mean as a mere matter of convenience to distinguish A, unborn, from A, born? When does function begin? Not till the baby cries? What nonsense it is! There is no life without function, for life *is* function. It is protoplasm doing something: being protoplasm in fact.

But if Roux is thus misinterpreted is it any wonder that we find biologists telling us with authority that though function shapes off and finishes what is furnished by genes and ids and heaven knows what, it only does it now and never did it till now? And what was it doing yesterday and what last year, last century, last glacial epoch and during the ages since two molecules, or more, put their heads together and became living matter with reversible catalysts making the function we call "life"? Can we not, for instance, use our imaginations and suggest that when the pre-living molecules were forced into conjunction they were "born"? If we might say so, then, but not before, function began. Yet the reactions of non-living matter showed what their work was. In doing it they

functioned as truly as when life began. We may, at last, begin to suspect that Verworn was right in saying the universe was "alive." The whole difference as complexity of matter and life increases is that the reactions are more and more liable to deflections, which may alter epigenetically the chemical and bio-chemical equations to which we may picture them theoretically conforming.

The Passage to Physics.—It seems hard for biologists to go back to physics. They tell us the passage thither is not yet charted. Maybe, but we can and must sound it. We are not wholly out of soundings nowadays, though fogs are plenty. It is a commonplace of the physicist that as energy runs down structure is built. This is the bound energy which some call entropy. The entropy of the universe is continually increasing, as available energy does its work and leaves structure and can do no more. This is the Second Law of Clausius. Entropy is the bound energy of Helmholtz. So a river cuts its bed, so a glacier builds its moraines, so a volcano piles its peak. In these structures is its bound energy, its entropic structures which determine, or very largely determine, future function. So it is with life, with living matter. The body functions and builds structures, and by its alterations and changes in hormones and catalysts and consequent changes in blood and lymph and all the processes of nutrition, determines how, and in what newly restored or changed ways, the offspring shall function and change and again build. Some surely must weary of hearing that function is acting in 1929 and that the functions of 10,000,000 B.C. had nothing to do with us. It has always been function which determined the path. And function then as now depended on the way of living forced by the environment.

Bone.—Bone is always a fascinating subject, and Hunter was perhaps the first who revealed to us what charms it possessed. As Huxley thinks it provides reasons for discrediting Lamarck it is small wonder that it comes

into this paper or that Roux's tropic theory of it is quoted. Roux accounts for the direction of bone fibres upon the lines that they do most good by a pretty application of Darwin's Natural Selection. Certain conditions of pressure and tension are most favourable for all growth and multiplication, and the bone is therefore strongest where these prevail. All other fibres are thus eliminated. The surviving fibres are favoured and everything is strictly accidental and a pleasant rider on the Darwinian Theorem. And when we analyse it we discover that we know exactly what we knew before, that is, that bone is constructed to carry stress to the best advantage. This was stated quite clearly by Wolff, and some years ago I extended Wolff's Law to muscle, an extension accepted by Keith (1919). Wolff, however, gave no reasons and with the help of Mayer and Culmann merely stated facts. Roux converted Wolff's Law into other words and pushed in Darwin and said, so we may guess, "Lo and behold!" But we may go much further than bone and muscle. All tissues at all times are the response, on mechanical lines, to function. For protoplasm is a great engineer.

The Osteoblast.—Can we not put the microscope of imagination on bone and look through it, hoping to see just a little more clearly what happens? It may not be possible to go far. But the first thing we can see is that bone is nothing but the calcium houses of the live osteoblasts, just as sponge is of spongeoblasts, and coral of the coral "insect." What is the osteoblast's business, what its function? Let us clear the ground by saying it is "a bone insect." Like the coral makers and dwellers it "wants" to live and divide and keep up its myriads, and an untold number of generations, beginning when cartilage, as it were, grew stiff with calcium and aged, have taught it how to do these things. It has learnt to keep the Haversian canals clear; it does not block up blood or

lymph streams : on the contrary, it lives where the activities of both, as well as its own activity, depend on stress. If stresses change what happens ? What of pressure, what of tension ? What happens in a bee hive when the comb tends to come down ? The bees strengthen the structure. So does the osteoblast, because if it did not its " house " would be destroyed, its " hive " would be crushed. And after all many are crushed. So the lumbar vertebræ of the weight-carrying labourer alter as the stressed osteoblast does its best against abnormal stresses. Give it normal ones, and time, and it builds where building is needed for a fair osteoblastic life and some freedom. It is a very busy " insect." In the human child it builds and pulls down where stresses no longer fall so heavily. For here as it cuts away the bricks and stone it laid we call it an osteoclast, though it is the same " insect " after all, unless, as sometimes seems to happen, it is too busy to divide and grows into a larger " insect " to carry more bricks and be quick about it, lest its new houses fall and there is a " bone-quake," as when adobe houses fall on human beings if an earthquake comes. Fanciful ? Where is the fancy ? Is it not an explanation of what must actually happen ? We do not need now to use Hunter's " absorbents." The word is superfluous. Had he known what Schwann and some before Schwann knew and taught us, would he, with his views of " consciousness," have called this fancy ? For all this holds good in all cases, those properly functional, or those morbid in origin, where at last an anchoring ankylosis is the forced reply to strange conditions.

For it is the body's internal environmental conditions as the external environment of bone which, within non-destructive limits, determine osteoblastic activity and function. They have learnt their business : they inherit the capacity of developing as their " parents " did in the same or very similar conditions. Theirs is a great business if a limited one. They grow again in each generation by

a true renewed epigenesis, a true new creation. There is nothing here to disturb any reasonable Lamarckian. But I shall return in another chapter to what Huxley has to say of bone.

"*Inherent Properties.*"—This biologist seems to believe that something is really gained when he writes: "I think we are justified in saying to-day that a very large proportion of what we may call the general adaptations of the body are due to the fact that during the period of development an equilibrium is attained between those organs which are functionally inter-related. . . . They all follow *en bloc* from a quite few inherent properties of the various kinds of cells which build up the body." Who—that ever thought at all—thinks differently, though it must be owned that "quite few" is rash. And yet one may legitimately be surprised to see so much acknowledged by an advocate of the thousands of weighable and measurable genes. This is a proper interpretation of epigenetic potentialities, though it lacks quite obviously any real suggestion of the machinery at work. What we want to know is in what way, when an organ is at work producing these "excretions" we call "secretions," these change and modify function and structure in other organs, so that they all can live and do work together in functional equilibrium. To talk of "quite few inherent properties" is merely to say they are able to do and be what they can do and can be. Elsewhere I pointed out some intelligible and perfectly obvious neglected factors in these problems. The neglect of pathology was insisted on, doubtless to a tedious extent, but I have said practically nothing about a branch of physiology which perpetually goes over the borderland of pathology and again returns, as it were, to proper physiological action. The subject of immunology is rarely referred to by biologists. Probably they know little more about it than their early textbook memories can carry them. That the animal body produces toxins

they know. That its structure must largely depend on how it deals with those which it produces normally they do not seem to have guessed. In *Malignancy and Evolution* I went far, or so it seems to me, towards proving that the one possible way of looking at organic secretions and excretions of all kinds is to regard them as quasi-toxic stimuli. That any gland working in excess or in dyscrasia has toxic effects we know well. Acromegaly and exophthalmic goitre bear witness to it. Deficiency diseases show that as the body has been built up by reactions to stimuli, their lack means at the least grave disorder. This disorder is probably due not only to the actual lack but to the comparative excess of other normal glandular products. How much we owe Starling is well known. We owe as much to J. T. Cunningham for seizing instantly on hormones as a possible mighty factor in evolution. But what are hormones? They are "starters." They rouse up the lazy: they initiate action. Shall we confine the word to a few recognized physiological phenomena? I think not. The body is built up by a great number of "colonial organisms." Basally they have what we may call their "social" or "tribal" preferences. This I tried to show when developing the theory of Hostile Symbiosis, which has received some recognition in quarters capable of judging its value or its worthlessness. The secretions of each colony are "poisonous" to itself. Their retention means the slowing down and final failure of function. They must be got rid of, just as a ferment must get rid of its typical product. They are also poisonous or toxic to other colonies. They must be met and dealt with, or lived with. As this was worked out elsewhere it may merely be suggested that until we add all such products to those already known to influence growth and function we are omitting what is, perfectly clearly, a real set of factors which should help us to understand that it is not by unknown "inherent properties" that a functional symbiotic equilibrium is reached

but by obvious factors with which any capable immunologist might deal. One side of this question which is seldom mentioned is that relating to the qualitative, quantitative and morphallactic results of secretions, autogenous or alien. It seems clear enough that the ante-pituitary has such powers. But in botany itself are to be found remarkable illustrations, such as the morphological effects of gall insects. A still stranger, but highly instructive instance is found in the Bull's Horn Thorn of Central America. In this tree ants eat out the central tissues of young thorns which if untouched grow straight, and live largely inside the cavities. The thorn then assumes what is usually looked upon as a specific character. Instead of remaining a thin, straight thorn it takes on the shape of curved bull's horns with a very wide base. The variations in this "specific" shape are negligible. This is an irrefragable proof that environmental agencies can dominate morphallaxis and alter functional growths.

Doubts about Lamarck.—After this Huxley has another shot at the Lamarckians. The facts which these say are the result of inherited functional adaptations are explained as being acquired afresh through the every-day function of the individual. Again we may ask when this process began. Huxley thinks that when Baldwin and Lloyd Morgan state that new developments in evolution often start as changes in behaviour and instinct, they do not go back far enough into ontogeny to cover more than a few of their facts. Does he go far enough back into ontogeny or phylogeny, or are we to be satisfied with a "quite few inherent properties"? But we may note with a certain grim satisfaction that after all he may have some doubts about the matter. For he agrees with an unnamed German that "until many more positive facts are obtained" with regard to Lamarckian descent, we must be satisfied with the creative effects of individual function. May we not say that this means such facts after all may

be obtained ? I recommend again to our Teuton and others a serious and unprejudiced study of anatomy and embryology with especial reference to the descent of the testes, the evolution of the stomach, the heart, and the uterus.

Chemical Skeleton.—Huxley, in a brilliant metaphor, speaks of the constant type of "chemical skeleton" possessed by vertebrates. Probably the basal chemical formula of each ductless gland product is alike, or even exactly similar, in all such animals. If we do not inquire how the ductless glands came into existence, we might indeed speak in this case of a few "inherent properties." But however constant this skeleton we have to ask why we get such different results. The real complexity comes in with differing proportions of endocrine products in a differing internal environment. We get, in truth, no further than if we talked of potentialities possessed by the primal amœba. This matter of differing proportions has been ably dealt with by Keith, who showed how a change in one would, and must, produce not only one change but a complex of inter-related changes. Huxley says that we are thus absolved from searching for reasons in function for many such complex sets of alterations. This is true enough, and when he goes on to suggest that H. F. Osborne's *Law of Uniform Development*, which is Orthogenesis re-stated, cannot stand, we may note that Huxley's later work on heterogonic differential ratios disproves the necessity for any such suppositions as those Osborne suggests. And here it may be said regretfully that Osborne's latest pronouncements on human descent undermine what authority he may have been held to possess.

Acromegaly.—In one respect at least Huxley criticizes Keith's view wrongly. Why is it that only some bones should respond to a late pituitary increase ? In acromegaly we get the inferior maxilla and the feet and hands mainly affected. Growth also occurs on the inner table of the skull. But Keith points out that these are very plastic

and unsettled bone areas. As Huxley constantly uses the supposition that many changes depend on whether their common structural antecedents have developed or failed to develop, there should be no difficulty in imagining it difficult for bone areas which have become static to start afresh. There may be some difficulty as regards the cranial growths, but if, as I have elsewhere suggested, we look on the brain and its case as a comparatively new and still rapidly evolving structure the difficulty disappears.

Hair and the Gonads.—There is also, it seems, something hard to solve on these lines in the growth of hair about the male human face. Why does the reproductive secretion not act all over the body? The answer is that it quite often does, and also that it once did. Perhaps it might be better to ask why so much hair has disappeared and why it remains most largely in the neighbourhood of the somatic orifices. Yet I have seen men with hair two or three inches long all over them. When they stripped they looked more hairy than many monkeys. We get some extraordinary hair growths in women. Why is it that the bushman of South Africa grows his hair in knots and clumps? Why are some dogs and sand-rats bald? Why do English fowls in Jamaica lose their neck feathers and become naked like the vulture? Why are the necks of vultures bald? Why do Polish fowls grow feathers in patches on their skulls, in which under each separate crest there is a hole in the bone? We might ask a thousand such questions and could only say in answer that each case is an "accident," or something due to highly complex causes as yet beyond us. We must put up for a time with not knowing. In any case male human face hair does depend eventually on the gonads. We do not know why. But then we cannot at once answer all the questions asked by Evolution, the great Examiner.

Uterine Growth.—We are also asked why the uterus and its appendages should grow during adolescence owing to

the ovarian secretion ? Why not the whole of the muscular system ? Huxley says we must postulate a specificity of the uterus to account for the facts. Perhaps so, but I shall have been much misunderstood if it is thought to satisfy me with a word, which merely re-states the fact. Let us try to get at the actual machinery. There is far too much shirking of such questions, for a very little examination ends in the discovery that the whole theory of genes and much modern Darwinism is merely a cloak of words for ignorance. At times we may well wonder why some of our most advanced teachers do not imitate the bacteriologists and tell us that Evolution is caused by Evolutionin.

Accidents to the Ovum.—This avoidance of real inquiry saves much time. And it also avoids the question of time. Again and again we are forced to note the common tendency to take a set of biological and physiological phenomena and try to solve inherent difficulties as if some organ had come into existence *totus, teres, atque rotundus.* Even embryologists are guilty in this respect. Some years ago I ventured to suggest that the origin of the mammalian must have been due to what we can only call a pathological accident. The primitive ovipositor failed to extrude an egg. Was this a minute favourable variation or a favourable large mutation, or was it for innumerable pre-mammalians a desperate calamity, an unevolutionary, and unprovided for, tubal gestation ? I have heard biologists try to wriggle out of the difficulty. They could not do it. An egg has to be in or out, and all the evidence shows that our early pre-mammalian ancestors laid eggs. The day, or the era, came when there was ovum retention. In the non-placentals birth took place before the embryo became a dangerous " tumour " as well as a parasite. For how long and with what a series of gestational calamities the safe-guarding reaction placenta was evolved we may guess from what we know of the Selachians. The proof that the action of the chorionic trophoblast is truly cancerous

has been demonstrated elsewhere. It was clearly stated by Adami and myself independently somewhere about the same time. Later Blair Bell's work seems to have been founded on this theory. But what has such a view to do with adolescent uterine reactions to ovarian secretions? Probably a great deal, for the very existence of placental mammals depended on a set of related reactions in related and closely conjoined tissues. Huxley has frequently remarked on the chemo-differentiative or quasi-hormonic powers of adjacent tissues. We can again look on the egg-bearing and ovipositor tissues systems as cell colonies "doing their best" to live and carry on. The early egg retention period was even more obviously a pathological one than modern child-bearing, though that, save in the rarest cases, is never without disturbance of function on the edge, or over the edge, of pathology. If the ovum at some stage before it destroyed the parent was not extruded it was destroyed with it. We can use Natural Selection here most legitimately and with great advantage, and even employ conjectural small favourable variations provided we look on them, not as chance favourable ones, but as continued reactions set in a line by the internal environment of an organism doing what it could to repair a disaster. These reactions were primarily concerned with the ovaries, the tubes and the reaction uterine pouch, or new tubal sac, and the placenta. The trouble may have begun, as I have elsewhere suggested, in a state of tubal catarrh as evolution on mammalian lines proceeded. It is true that danger did not occur until the retained ovum was fertilized, but it is not at all difficult to imagine that the early tubal congestion and obstruction reacted on the ovary, so that after many cases of trial and error the ovarian secretions incited the start of uterine muscular growth. If some such mechanism did not arise out of the very gradual physiologization of an essentially morbid state we cannot imagine a mammalian at all. It seems that if

we explain adolescent uterine growth in this way the question is simplified and no longer mysterious. For this is continuous hereditary epigenesis on mechanico-physiological lines. Another mystery might be made and indeed has been made, as to the great increase of uterine muscle preparatory to birth. The explanation here should be simple. The uterus is an enlarged part of the tube. The tube propels the ovum by its rhythmic contraction and expansion until it reaches the newer evolutionary sac. To what extent the Fallopian tubes and the uterus before gestation are always in such an alternating state it is hard to say, but we can affirm that the continued painless contractions of Braxton Hicks during gestation, a modified effort of expulsion processes, are the very definite cause, or antecedent condition, for the increase of the uterine muscle. In this way it is exercised till, on a continued increase of power and increase of the irritation of the enlarging fœtus, expulsion does at last take place.

Now let me remark that I am not here laying any great stress on this explanation of adolescent growth as necessarily correct. It is far too revolutionary in its open recognition of disaster as a factor in evolution to be accepted without great reactions among the opposing school. What I do lay stress on is that this, or something like it in its frank recognition of a tremendous time factor, is the kind of explanation we want. No good whatever can come of handling the facts as we see them now, even as we see them *in embryo*. We might just as well try to solve the problem of the yucca and the Pronubis while ignoring the high probability that the beginning of the relationship between the plant and the moth began millions of years ago, when the yucca was no yucca and the moth a primitive member of pre-lepidopterous ancestry before the Homocera or moths were in existence.

The Alpine Dandelion.—And now, having come, as it were accidentally, on a problem in which botany is clearly

concerned, I am moved to make a slight examination of one more argument against Lamarck advanced in *Animal Biology*, that other interesting new book by our young biologists, Haldane and Huxley. To look at such an argument with something not unlike a smile will not, I hope, be taken to indicate that for much or most of this work I have not the greatest admiration. This is due, it may be, to reasons which would not commend themselves to one of them at least. The reason is that the notable and valuable experimental work done on tissues *in vitro* by J. S. Huxley seems to lay about as much stress on internal and external environment as to satisfy so strong an environmentalist as myself. But let us look at Haldane and Huxley's dandelion (*Leontodon*) when growing in the Swiss Lowlands and high up in the Alps. In the book it is said that the resulting differences, which practically amount to Mendelian longs and shorts, should, according to any Lamarckian hypothesis, have been perpetuated. The same example of false reasoning is to be found in *Living Organisms* (Goodrich). The fact that the Alpine type, when taken down and grown in the valleys, at once takes on the lowland type is held to be a disproof of the theory that the direct action of the environment can be perpetuated. Now this statement contains more than one fallacy. That these differing examples of the plant suggest the Mendelian scheme and that two dandelions in neighbouring positions may, owing to different mutations, show large and dwarf varieties, is a very fair proof that no such changes as the Lamarckian posits have ever taken place in the germ. In any case these are but varieties and show no specific differences. But another and most important point has been left out of consideration. It is well known that in Alpine regions there is a cold down draught during the night, and during the warm day an ascending current. It follows without doubt that the floating seeds are constantly in motion during the seeding

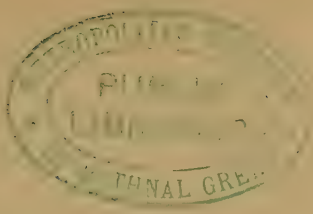
so that the Alpine plant's seeds may easily fall in the valley, and those of the lowland plant may rise to a great height. Where separation and great variety can be seen and there is this perpetual interchange, no Lamarckian, or Neo-Lamarckian, or any brand of environmentalist would look for essential or specific difference. But when we consider such points it is as well to remember that no one nowadays interprets Lamarck's language in the spirit of a lawyer interpreting a will, when the obvious verbal meaning must stand. There is nothing now written about and recognized on the subject of the effect of function in form change which Lamarck would not in all probability have accepted at once. This is, assuredly, how he would now interpret what he said of habit and need (*besoin*). If the Neo-Darwinians should complain that Neo-Lamarckians have shifted their base, as one might say, by night, they may be invited with all cheerfulness to reflect that Darwin was infinitely nearer Lamarck than he seemed to believe and that the magic modern scheme of evolution with use and disuse left out would strike this great and partly deserted master with much amazement. Those who doubt this may, at any rate, refer to Samuel Butler's savage and unmannerly attack on Darwin. At Butler's constructive work we may well shrug a sceptical shoulder, but that he had an acute and critical mind cannot be denied.

Steatopagy.—I note again, close to the section which led to this excursus, another tendency to words which seem to explain and yet make nothing clearer. Huxley asks how endocrinologists can explain the feminine Hottentot steatopagy "without assuming a special and inherited sensitivity of this region of the body, in the shape of a selective tendency to the accumulation of fat?" Such changes, he says, cannot be due to quantitative changes in hormones. "We must appeal to genetics." The pigmentation of tropical races, too, cannot be due to

endocrine changes. Well, let us note that Huxley shows pituitary extract can turn a frog or a salamander as black as coal (pp. 292-3). So if the adrenals cannot do it the pituitary can, and for all we know it may do it in man. I am, however, not so much concerned here to defend the endocrinologists, who seem well able to take care of themselves, as to criticize such a phrase as "a special and inherited sensitivity." Why not a special and inherited local insensitivity to thyroid action? Do we not in thousands of cases get steatomata and lipomata? It is true we have not explained their local occurrences, but the fact that they are local is here relevant. We are not entitled to say their locality is dependent on genetics. It would not be absurd to say that we do not yet know anything about it. And since it is clearly insensitivity, not sensitivity, which is in question we can scarcely ask the genetic philosophers to state that advantageous mutations of less sensitivity can be acting here. Still I own that they may, and do on occasion, say anything. Huxley commits himself to the very rash statement that there is nothing like hormone action in insects. He relies on the stated absence of the sex hormone and puts down all secondary sex characters as due to the chromosomes. Thus accidents to these may give rise to a gynandromorph. But this sometimes happens in mammals, in man, and in cattle as "free-martins." And we may add that even the maddest Lamarckian believes chromosomes can do a very great deal. He and I, for instance, believe they can do so much because they are part of the hormones and catalysts with which cytoplasm, and nucleo-plasm, if there is any, do their work as influenced by stress, function and the whole internal and external environment. I may remark here that the studies of Boveri (1906) on non-nucleated fragments of *Sphaerechinus* showed that the resultant plutei possessed quite as large nuclei as those from a nucleated fragment. "It seemed then that the supposedly non-nucleated pieces contained the

normal number of chromosomes!" The cytoplasm is assuredly underrated by some. The nucleus probably possesses a mere selection of the general cytoplasmic tools or dispersed chromatin.

The Glands of Swammerdam.—With this we may leave the frog and the tadpole. I do it with regret. The book is really interesting, and those who learn that the brilliant young biologist who wrote it has been drawn into other work than the research for which he is so well fitted, do so with the greatest regret. Should he ever return to his proper sphere, the laboratory, may I beg him to give still further attention to the frog with a view of explaining a greater problem than those he has tried to solve. If he can tell us a convincing story of the Glands of Swammerdam, their origin and the meaning of the strange overgrowths to which they give rise, without summoning djinns or genes to his aid, he will earn the lasting gratitude of the zoologic world.



CHAPTER SIX

PROTECTIVE RESEMBLANCE

The Nature of Naturalists—Scent and Selection—Knowledge as Reaction—John Hunter and Consciousness—The Real Judge in Science —"Mind" and Pavlov—Forced Variations and Adaptations—Brain and Work—Adaptation and Locality—Blocked Function—Colour and Excretion—Inertia of Variation—Safety and Location—The Eclectic Upshot—Biochemistry of Colour—Colour and Sunlight—Dread of the Sun—Man the Hunter—The Springbok—Sexual Selection—Zoology and Pathology—The Rattlesnake—Herds and Evolution—Real and False Protection—Truth and Variation.

The Nature of Naturalists.—It is well known among the politically inclined that no question is so likely to rouse the evil passions recessive in all of us until they become dominant, as the subject of Protection. It is a curious coincidence that nothing coming up for discussion among zoologists of all degrees is so certain to incite fury and its concomitant unreasonableness as the problems of protective coloration in animals. To anyone of a ribald disposition combined with uncontrolled humour the results are sure to give almost unlimited satisfaction. W. H. Hudson used to relate with gusto that he had seen a well-known entomologist come into the reading room of the Zoological Society and, finding on the table his latest book, handle it with obvious satisfaction. He then noticed the new work of a fellow authority on the *Insecta* and, taking it up with an expression of some distaste, turned over a few pages and put it down, saying quite audibly: "All wrong, all wrong!" To Hudson's extreme delight the rival writer soon followed on the heels of the other, and gave a really remarkable

display of pure reflex behaviour by going through an exactly similar course of action and speech. Now Hudson did not know what particular point in each book was looked on as "all wrong," but I am strongly inclined to believe it must have been one connected with "protective resemblance." Assuredly no other subject rouses writers to utter such insults as modern manners permit. What they would have said of each other in the days when a greater latitude allowed great men to exchange compliments now confined to the gutter, it is almost impossible to imagine. Yet if other times bring better manners and more politeness they also bring thinner skins. When anyone given to systematics strays, as the field naturalists seem to think without any right, from skin and bone to the open air, he is sometimes unkindly referred to as "a closet naturalist." To say that the learned authority thus referred to is apt to foam at the mouth if he comes across these seemingly innocent words may appear an exaggeration, but when in his reply he refers to them with anything but cryptic bitterness and rakes his mind for epithets implying insolence, audacity and ignorance combined in those who disagree with him, we may picture him in the seclusion of his working-room almost in a state of frenzy. The spiteful field naturalist is often far more calm, for being aware that "closet naturalist" carries with it the worst poison in the whole armoury of contention, he can rest more or less satisfied by the symptoms the phrase provokes. Such an assumption of superior knowledge and vaster opportunities for actual observation seems to place him so far above the blind beetle-like systematist that he can go on observing in comfort and satisfaction. Perhaps the worker in museums has too often brought this exceedingly unfair attack upon himself by denying, openly or by implication, that the observer is really a man of science at all. This was undoubtedly wrong of him, for a field naturalist is deeply interested in that part of scientific work which

necessarily precedes in time that done by those workers who have to establish order in the facts gathered by the labourers they often look on as their hodmen. As I have often said, it has been my own custom when considering any scientific question to take the position, until forced to abandon it, that no man with brains can deal during his whole life with any subject and yet be entirely wrong. It is true that some men may be interested in a subject and go off the track altogether. The absurd battle between Hobbes and Wallis upon mathematical points is well known. Hobbes was undoubtedly a man of gigantic genius, but his mere interest in mathematics did not produce for him the normal results of continued application. Thus Wallis not only walked over him but trampled him in the dust and, as it were, whipped him with equations as though they were scorpions. But a real life-long application to definite studies should always meet with some respect. That the field and closet naturalists do not always agree is, of course, natural, since no naturalist agrees with another, or ever will. What both will think of an outsider who proposes to inquire here whether they are not both right and both wrong when they squabble so fervently about the theories in fashion as regards protective resemblances is a difficult question. If an unorthodox theory emerges, that again might be a source of scornful criticism, as I have no pretensions to be a man of the " closet " or the " field," though I have had great and admirable friends among both.

Scent and Selection.—The shifts that many animals are put to in order to obviate the disadvantage of a strong scent are well known. But it may be doubted if they are as many or as ingenious as those resorted to by orthodox biologists when they try to get away from the fact that there are such things as unfavourable variations which are not, as Darwin incautiously suggested, immediately wiped out. The part that Natural Selection played, is

playing and will for ever play is by no means always clear. Those living creatures which do not so move as to avoid, or so vary as to meet, dangerous assaults of the environment, certainly in the end perish. But nothing forbids us to believe that a disadvantageous variation may not only not cause death at once, but may go on increasing, provided its detrimental effect is offset by other variations in habit or structure, or in both. Perhaps purely scientific biologists almost invariably tend to a hasty over-simplification of the facts. That Darwin, in the flush of his great discovery of the principle of Natural Selection, should have overrated its effects is natural enough. He was dealing with an unco-ordinated mass of chaotic material which had till then defied any effort to put it into real order. When he saw the possibility of order emerging what more natural than to think the work practically done? His successors having the advantage of his herculean labours in the Augean stable of natural history as it was then taught, had ample time to see, however certain the fact of evolution might be and indeed was, that his machinery was insufficient to account for many biological phenomena. Yet, instead of adding to his apparatus, they have proceeded, under the destructive influence of Weismann, to reduce it. Even now they deprive him of use and disuse and thus of all somatic inheritance, and having in this way skinned their prophet they rely on causeless and Mendelian variations and Natural Selection alone to explain the phenomena of life. No one can be more hostile than myself to any interpretation which smacks of vitalism and predestination, but if we are to be deprived of everything but the tools offered us by the Neo-Darwinians we may almost as well sink into theology at once. Is there anyone who can seriously deny that scent in a deer or a hare is anything in its essence but a grave disadvantage? Why has not Natural Selection eliminated it if it can do so much in protective resemblances? For we may reflect that

dangerous animal scents are often so gross that they can be readily recognized by the generally inefficient human olfactory centres. Animals with wonderfully acute sight and hearing do not need dangerous smells as recognition signals.

Knowledge as Reaction.—It must be clear that many of the views put forward conflict not only with the primitive psychology of the average man but even to a greater degree with some professional psychology. When speaking of the average man I mean the average intelligent man who thinks any discussion as to what we mean by "knowing" not mere foolishness. This is not any prelude to the metaphysics of epistemology, but nevertheless we may discuss what we and animals know and how we and they know it. As a rule the less people have read upon these subjects the more ready they are to admit that animals and insects are not in any way automata, but that in their own range they think and feel and reason and act as logically as ourselves. In *Animal Behaviour* it seems that Lloyd Morgan goes as far as that. But will it seem absurd if I say that they are, on the premisses from which they reason, even more practised in logic than ourselves? Their range is limited and can therefore be learnt far more easily than ours. They react to it readily and without much confusion or doubt, both of which in us come from the complexity and disorder of our knowledge. Now it must be admitted that logic is often assumed to be a somewhat magical way of finding out things. But how did it arise that we can put into syllogistic form the fact that two and two make four? We can do so because of our experience. There is nothing in the nature of things to suggest that it must be so. Such a fact has become of importance to us and we therefore have learnt it and know it and act upon it. With animals it has rarely been necessary for them to learn it and certainly they rarely learn that five and one make six. Yet many animals are perfectly aware that one minus one is nothing,

and those who doubt it need only take a lamb from a ewe, or a single puppy from a bitch to discover so much. When we hear of savages who can count only up to three or five, we really learn nothing of their arithmetical powers. I might regret it if I stole half his catch of fish from a " savage " who had caught a dozen. These remarks might seem idle and they would be so if it had not been for the complex fakes of psychologists who still turn cerebral reactions into unintelligible verbal machinery, and invent all kinds of explanations for the simplest behaviour. The ordinary dog or cat owner often seems to me to be better prepared to take the right psychological sow by the ear than the professional expert. He will be impatient with those who ask whether his dog thinks or not when that animal, usually eager for a walk, makes no motion when he sees his master take up a bag of golf clubs, while a moved walking-stick makes him bark with joy. If such a man sees an insect try to do something and fail, and try again and at last do it, he may listen to talks about blind instinct without visible impatience, but inside he is, perhaps without knowing it, quite sure that the insect is aware of the difference between success and failure, and feels reasonable satisfaction when it does what it so obviously wants to do. He sees no hard and fast line between knowledge and instinct, and if he is at all tinged with physiology and neurology he might say that continuously inherited reactions, developed by function and trial to perfect accuracy, were more certain signs of " getting there " than we possess. He might even believe that what the insect even now does by " instinct " was first done by the processes we call " learning how to do a thing." This is now admitted by many. We may deduce it from Pavlov's conclusions on the nature of instinct as compound conditioned reflexes. This is the way we all know, even if human reactions as conditioned reflexes are much more complex. My ordinarily intelligent man would make many wrong assumptions. He would probably assert that the

actual knowledge acquired in one generation was directly transmitted. He would be merely a natural but over-devout Lamarckian. Yet a little help might enable him to see that what has been inherited was not the knowledge, but a slightly altered organization which reacted in increasingly "intelligent" enforced behaviour in the presence of like stimulation and inhibition. He would not believe that the insect was not properly "conscious" if we define consciousness as the spread of stimulation from the centres first stimulated, a spread by irradiation as Pavlov calls it, because it acted almost inevitably, any more than he would believe that he himself was not conscious when he moved towards the dining room when the gong sounded, or away from the fire when it was too hot for comfort. He would never fall into the refined folly of believing that words are necessary for the act of thinking.

John Hunter and Consciousness.—So much seems necessary if the general thesis, that animals know tremendously well what they are about and therefore make use of any change in their environment and also in their bodily construction which is not too disabling, is to stand any examination. This is all compressed in the statement that adaptation is not always the somatic change itself, but may often be the search by the altered soma for means to live and a place to find those means. It is true that accident may help and sometimes be the greatest factor, but the essence of the whole thing is that knowledge is the means of useful reaction and that possessing useful reactions is having knowledge.

How disagreeable and even contemptible such views must seem to those who cling to the portmanteau conception of mind, soul and spirit is clear. Consciousness is for them not truly a cerebral, not a somatic, state at all, but some expression of "mind" which reaches its acme in man and is, perhaps, found in him only. Long ago—long before behaviourism as a term was invented—I came to the conclusion that such a view was so false that one wondered how it

was ever accepted. We may say that it never did arise at any given period. It grew up in us all, for were we not the last wonder of the world and the greatest, and, therefore, distinguished and distinguishable from all other living things? It is in fact a survival and stands alongside vitalism, spiritism and a thousand other savage beliefs, in which we may include the belief in consciousness as a cause rather than as an effect capable of definition.

Now who was the first writer who suggested that consciousness, in a very true sense, is the appanage of all living matter, even in the earliest stages, and thereby initiated a movement which must lead, and is now leading, to a total reconstruction of our notions of all behaviour. This must mean that the whole magical apparatus of the pure psychologist, or psychic word-monger, must be relegated to a museum. That writer was, I believe, John Hunter, perhaps the greatest intellect seen in Great Britain since the time of Hobbes. For Hunter in one passage, neglected by almost everyone but Keith, speaks of tissues possessing "a consciousness of imperfection," and in a second uses words intelligible only if taken as implying that bony tissues "knew" what they were doing. It is easy to dismiss the words of John Hunter as mere figures, but those who have tried to understand his methods of speech and thought will be the least disposed to do so. Let us remember that Hunter was impressing his mental stamp on the whole medical world in a time when the cellular constitution of living matter was unknown. But a tissue for him was something that lived, that had in it and of it "a vital principle." It should be emphasized that by this he by no means implied here that life was a thing imposed from the outside, a view I have but lately seen falsely attributed to him. His comparison of life and matter to iron and magnetism shows clearly that, whatever the imperfections of the language he used, he was by no means a vitalist as we understand it. Moreover he distinctly

stated that when he used the word mind he meant the brain. If Schwann had been a contemporary of Hunter's and if Hunter with a competent microscope had seen those new cellular units of life, it is not hard to imagine that the reactions of the fixed and mobile cells thus revealed to him would have been viewed with intense satisfaction. Would he not have said, "Why, before I knew all this I still recognized that the tissue web of which I could but guess the constitution was not only alive in a narrow sense but also had consciousness, by which I mean a power of knowing its business, and here I see those cells, of which I knew but the results, going about that business with a strange and marvellous automatic precision"? But I may later develop more fully what can be said of Hunter's views.

The Real Judge in Science.—For this is, we may say, the very *fons et origo* of that physical and psychological behaviourism which is slowly but surely replacing the guesses of the verbalists. For it great thanks to Pavlov. So far as I am aware there was never any reasoned and completely carried out attack by men of science before Pavlov on the position of those who maintain that the philosopher, who endeavours to attain precision of thought by the employment of words which mean one thing to him and something else to the pundit in the next street, has the right, and the final right, to judge the labours of those who have given their lives and their labours to scientific advancement. No doubt such men scarcely care to combat this view. Whatever the differences among scientific workers they are as a body committed to the belief that they must give results which fit what others have made at least fairly certain and that all their conclusions must be put into words and weights and measurements that mean the same in London and Edinburgh, in England and in France, in Europe and Japan. Knowing this and seeing, as they must have seen, that no two philosophers agree upon the meaning of words, and that every one of them is as likely as not to put a word

into a major or minor premiss with one side of its connotation uppermost and take it, when capsized, into his conclusion with the triumphant air of a conjuror, they may very well have shrugged their shoulders as they ignored the verdicts of these self-appointed judges. It can, however, now be maintained and it is, indeed, daily becoming surer, as experimental psychology and the allied sciences advance, that philosophy and metaphysics of all kinds are as much behaviourism of certain types of brain in the face of the unknown as the actions of any insect or animal. They thus become part of the province of action native to the man of science. It is he, not they, who must in the end be clad in the ermine. And this is to say that none of us has the right in science to go any further in analysis than definite words, weights, and measurements will carry us. I cannot help thinking that John Hunter would have subscribed to this, although I have tried to develop the thesis not so much to glorify him as to give definite reasons for dispensing with words like " consciousness " and " knowledge " and " knowing " when they are held to imply more than the reactions, however simple or however complex, of living matter, in a single cell or in Socrates, to the environment. For when taking this view we can see how futile and meaningless it is to deny to all living things the capacity to deal with what they are acquainted with, that is, with what they " know."

" *Mind* " *and Pavlov.*—This way of looking at knowledge fits all the facts of life far better than any conception of dealing with the environment which requires us to posit some indeterminable entity such as " mind." For we can only arrive at such a conception by going further than the facts warrant. We must rest content with " behaviour " for man as well as for the rest of life. Probably few real behaviourists reserve more for themselves than they find in our lowly companions in the world. It is worth noting that though Pavlov states that he did not reach his conception of conditioned reflexes by reflection upon behaviourism

it is now understood that it fits in with conclusions based on the pure study of conduct. What induced him to study the question and to arrive at results, which are now beyond anything more than captious or religiously prejudiced criticism, were the gross difficulties he found in using the hackneyed, overloaded, and often totally meaningless, words found in psychologic armouries. He says: "An unqualified conviction of the futility of subjective methods of enquiry was firmly stamped upon my mind." (Pavlov, 1927, p. 6.) By overloaded I mean that into an apparently simple word enough meanings were packed to satisfy a Bateson among philosophers. They meant just what the writer wanted them to mean and when he waved his magical wand they meant something else. It is, perhaps, not travelling beyond the record to say that fifteen years ago in unpublished work of my own I defined conduct and behaviour as the result of what I called "reflex cerebral arcs," being led to do so by much the same difficulties as led Pavlov to undertake his long and richly rewarded search. But, as my own tentative conclusions were reached a priori so far as experiment was concerned, they remained valueless except as verbal criticism and were not printed. It may be added that J. M. Sechenov in 1863, and Ch. Richat in 1925, attempted constructive work on these lines, as Pavlov remarks in his first chapter.

Forced Variation and Adaptation.—If these views are accepted there is no difficulty encountered when we state that animals not only "know" in the same way as ourselves, but having regard to the comparative narrowness of their environment and the longer time they have had to learn how to deal with it, they really "know" better. To look on animal life in this way relieves us of the difficulties many naturalists appear to find when asked to believe that any new difficulty among the lower animals may be met as we meet one. A very little consideration of the futility of man in the face of the unknown, and even the comparatively

new, will increase our respect for animal " mind " and tend to diminish the awe-stricken respect we have for our own. We recognize that our own instincts are knowledge which has become reflex by a fixing and limiting nervous structure and that instinct in animals, though often more certain than ours, is of exactly the same nature. We should therefore have no further insuperable difficulty in believing that animals, when altered by forced variations, can so choose their place that they seem the subject of adaptation. But this must not be taken to mean that the variations in question are Mendelian mutants of a definite character arising we know not how. The word " forced " necessarily implies stress and reactions consequent on pathologico-physiological lines.

Brain and Work.—It must, however, be owned that when we take into consideration the general views of mankind, from which some few scientific men are not yet free, which place humanity not merely on the highest pedestal but on an entirely solitary one, we need not be surprised by anyone still taking the view that animals and insects have no real brains but are practically mere automata. Many feel that animals without our nine thousand two hundred million cortical neurons must necessarily be fools without sufficient knowledge to " come in out of the rain." But do our neurons preserve us from folly? No, too frequently they merely open fresh paths for error. Whatever the number of an insect's cortical neurons, if it has the minute homologue of a cortex, its nervous organization does, as a matter of fact, fit it for its job, and if it did not it would take another or go under in an environment which it could not handle. Brains and general somatic construction may differ but they do their work. A bird's lungs are adherent: it has no pleural interspace, but we do not therefore say it cannot breathe. A hen bird's left ovary is the only one that functions: the right is atrophied. But that one functional ovary enables it to lay sufficient eggs to keep the race going.

And the animal's brain does the animal's work. What a pity it is that ours are not so sure and automatic in response! So we should not underrate the intelligence or even the vaso-motor emotions of other animals than ourselves. They are not such fools as they may seem, and we are bigger fools than we commonly imagine.

Adaptation and Locality.—It follows that when we find a sandy-coloured bird in a sandy-coloured environment we need not instantly infer that Darwin's scheme, or Lamarck's, fitted it for such a habitat. Both may have helped, but what forbids us to believe that such a bird with hawks for its enemies would know perfectly well that it was safer there, at least when nesting or at rest, than in a bright green pasture? Let us imagine that, being yellowish, and who knows really how it became such? it found it was worried in one environment and peaceful in the other and had the sense to stick to the place that gave it peace and freedom, though it may be at the price of having to change some habits and perhaps its favourite food. And if another bird lays white eggs in shingle it is rash to say that it did not see the safe likeness between eggs and pebbles. There is no need to argue that Natural Selection preserved the white varieties of egg. Is a tern a fool? I admit that there is, in North America, a "fool hen" which seems to deserve the name. But, curiously enough, it is only a fool so far as man is concerned. Its colour and habit as it sits on a branch protect it from all but human enemies and that fact has sunk so deep into its nervous organization that it will remain to be shot at a dozen times. But as regards birds and white eggs in white shingle the opinion is certainly growing that a bird can know, as well as we know, that small whitish pebbles may look like eggs from a very short distance and is perfectly able to infer that eggs may therefore look like pebbles. Like ourselves it probably made mistakes before it learnt as much. So we may be perfectly prepared to believe that some birds which lay white eggs have been influenced by that fact in

the choice of their nesting-places, though we must admit that the absence of sunlight, affecting both the eggs and the parents as they sat so long in the dark, very likely affected the coloration of such eggs as those of swallows and martins.

Clearly this is all strictly relevant to what we call adaptation, a subject which has become a battle-ground in which the combatants can scarcely see each other for vain dust. We have, truly, little right to use the phrase at once when we find any organism fitted to its environment. Earlier I named L. Cuènot as having put forward a like view, though my own conclusions were reached independently. For we have no evidence that it did not survive there merely because it went there and found by experience that it fitted the place it chose. A hermit crab tries many shells and takes the best. This is not a new view of part of the facts concerning "adaptation." It is exceedingly difficult to discover what some really think of the subject. An increasing distrust of orthodox explanations has made many uncertain but still not ready to discard them openly. Yet as soon as it is acknowledged that animals change in form and colour from obscure photic, bio-chemical, nutritional, and endocrine reasons, most of which present knowledge cannot yet classify or elucidate, we are free from the necessity of explaining these prime changes by the mechanisms of Darwin or Lamarck. If, then, we say that an animal or insect finds a place which on the whole suits it, we may infer that it does well owing to combined accident and knowledge. I say "accident" because a fitting locality may not be easily at hand. If it finds it, then, indeed, we may call in Darwin. For Darwinian accidents of variation, if they occur, will aid the animal, though it is obvious that, on the whole of the lines suggested, further variations will mainly depend upon the very fact that it does do well there. For doing well means ease of nutrition and safety in fertile reproduction. To say so much of course allows that others will do less well and that implies Natural Selection.

Blocked Function.—Those doing less well must, if they survive at all, alter or increase some of their functions, which, by the new direction of energy, must block other functions or negative other potentialities. If they do not perish they will change. But they do not survive because they have changed: they have changed because they survived owing to their reserve plasticity and powers of differentiation and de-differentiation, that is, by their capacity to change by use and disuse, with re-directions of energy. If we think of coloration on these lines and recollect that colour requires pigment, pigment-cells or chromatophores, or energy devoted to developing structural colour, we can see how easily extra energy will work in the case of animals in safe circumstances and how difficult it will be for hard-pressed ones to devote part of their energy income to adornment or somatic change. We infer, moreover, that colour patterns, though they might be changed by so-called accidental minute variations, were assuredly not initiated by them. It should be remembered that any colour must be regarded as the outcome of some deep internal change and is therefore likely to increase when once started. This seems a general principle of all growth. Darwinians of the old school, who have not succumbed to the magic piping of the gene, may easily continue to believe that given a beginning on these lines, Natural Selection may advance it.

Colour and Excretion.—Colour changes are variations, and Darwin owned his ignorance of their origin as he did of all variations. But nowadays more is known of them than in his time. Apparently he knew nothing of catalysts, of enzyme action, or of the function of unexcreted katabolites in colour production. How many dogmatists know much of them now? In many cases these products seem to drift into areas with a low metabolic rate, such as the wings of the Lepidoptera, where they are excreted or finally dissipated with epithelial dust. This means that what they achieve in colour is accident and a method of deferred excretion. I

forbear to inquire how far this may be looked upon as a pathological process, implying inadequate nephridia. But when we know that urea is actually used up in this way such an inquiry may not be vain or otiose. We may then, in many cases at least, account for colour ornament as deferred energy products. That its production must follow, so to speak, equational bio-chemical and chemic lines in accordance with structure, is certain, and if we accept the excretion view we shall feel sure that the highly coloured animal is usually not only well-fed but even over-fed and therefore abundantly supplied with energy to stand the strain of inadequately developed excretion. Most of this abundant energy will be spent in sport, play and reproduction, but any excess must be spent in structural development. How well this view fits the fact that ornament is almost universally the overgrowth of a useful part of the insect or animal, can easily be seen. We can, if we will, carry inference further, and say that related animals or insects in such an environment are likely to develop on like lines and therefore to resemble each other. As said earlier, this has, therefore, a possible relation to mimicry and the acquisition of like catalysts.

Inertia of Variation.—It may possibly be admitted that such an explanation of ornament and coloration, say in a butterfly, looks reasonable enough if we are not deeply committed to other views. For it does not imply that new or altered characteristics are directly inherited. All it means is that in the same morphallactic environment similar modifications will tend to arise and tend by inertia to continue in inheritance. Natural Selection can be used when it is desirable, but, with Darwin's full assent, we may and must employ use and disuse as well, and we do that when asking assent to the theory of energy direction as of the highest importance in evolution.

Safety and Location.—Much of this may seem to have little relevance to the supposed changes tending to protective

coloration. To my mind it has much. Though I believe with most field naturalists that age-long continuous exposure to sun is likely in the end to bleach colour, I am bound to believe also that where any " accidental " change in that direction has occurred animals learn to use it, just as they learn to keep still when motion may discover them, though they have, it seems, great difficulty in keeping their ears still, a fact which W. H. Hudson in *A Hind in Richmond Park* might have added to his curious observations. And while speaking of him a line or two of that chapter may be quoted. " A life-long intimacy with animals has rid me of the common notion that they are automata with a slight infusion of intelligence in their composition. The mind in beast and bird, as in man, is the main thing." And by the " mind " Hudson obviously meant the brain. He was not what may be called a " mentalist," though I own to some reluctance in using such a hybrid word. As remarked earlier, we may go further than Hudson and say that animals, in their own environment and at their own job, frequently show more sense than men, who have not sufficient sense to interpret what they ought to see plainly. For do not many observe their pet dogs and cats reasoning far better than their curate, or even a dean or bishop, and yet maintain that these clerics have intellect and that the animals have none ? But what again are cerebral cells, and a nervous system with its evolved and evolving reflexes, for ? They are to enable the animals, man included, to act with some safety in the environment to which they belong. Mark, not in any other, but in their own. And with non-predaceous animals it is always " safety first." That is their main preoccupation and they are good at the job. Do men act with anything like the same certainty, and with what we call reason, in their environment as animals do in theirs ? If they did we should never have had a Frenchman, who no doubt knew he was as big a fool as his fellows, writing *L'Homme Stupide.* We must take

it for granted that animals reason well and are not fools and therefore go where they are safest. So whether colour is forced upon them by a long process or by an accident they will find out its possible uses. It may even be conceded to the Mendelians that a violent colour sport might alter habit strangely. But it would do so because the subjects of the change then chose perforce a new or altered line of life.

The Eclectic Upshot.—The upshot of the whole matter seems to be, as is so often the case, that all the schools are right and all are wrong. Is it possible to arbitrate between them? If it is the decision will surely leave many unconvinced. For the nature of men of science is human nature, and it always wants its own way and all its own way at that. Yet assuredly when we survey as far as possible the whole of the facts there is much to be said for all the combatants, and there might be more if they would but instruct themselves somewhat in branches of knowledge they forget as they try to solve, by simple inspection, problems that might at least be referred to sciences which they ignore.

Bio-chemistry of Colours.—In the first place it must be recognized that colour is essentially a matter for chemists, bio-chemists and physicists to discuss, and we must ask them to call in physiologists and endocrinologists as well. What the systematist, or the field-naturalist, says cannot be final. Pigmentary and constructional interference coloration presents problems not to be solved on inspection by any eye, however keen. What we want to know is how it arose and what catalysts and enzymes are concerned and what part was played, and is being played, by photic stimulation and by its comparative absence in the shade of forests. We may then learn why some Arctic animals grow white, not as a protection, though they may use it as such, but as a result of a set of forced endocrine reactions. Some such study might conceivably reduce much assurance to comparative humility. For

what we actually know of the effect of sunlight on colour production is small indeed, even if we do see that its intensity and its variation in intensity have varying effects. If its intensity compels chromatophores to take shelter and finally perish, as may be suggested in some cases, it would be interesting to learn why in others they flourish. Perhaps they, like muscle cells, fail under continuous extreme action and flourish under rhythmic conditions of action and rest. But, on the whole, it is possible that endocrinologists, when studying the action of abnormal pituitaries and adrenals, may give us most help. Addison's disease may teach us more than it taught Addison.

Colour and Sunlight.—Independent of such investigations, which are still in their bio-chemical infancy, or perhaps gestation, we may, however, take for granted that the direct action of the sun in vast open spaces such as Africa has possessed for a very great period of time, since the geological period of great lakes in the south of that continent passed away, must have had its effects. Before that era commenced it seems that Africa must have been one great forest, such as exists over large parts of it at the present time. It was in these forests that the African fauna, in all probability, arose and differentiated. This we guess, as the young of most are still dark, speckled and striped and spotted. Did their dark ancestors die out at once when exposed to the sun, or were they not gradually changed, generation by generation, during millions of years ? We know that immediate and complete exposure to a burning sun might easily destroy some forest animals. The medical profession recognizes now that while sunlight is infinitely valuable it may in excess be highly dangerous and destructive. Why, then, did not the race of men get bleached if the animals did ? To have to ask such a question shows how little we know of a subject about which so many are so confident. The action on skin epithelium and on hair seems different. We often see human hair

bleach under the sun. Many women have hair on sheltered parts of their head which keeps the colour it had in their youth. Even skin does not react as we might expect it. Many white men tan almost to blackness; in other cases, not always those of obvious illness, a peculiar pallor supervenes. I suggest that there is an endocrine basis in these cases. As regards the negro what are we to say? Probably the sunlight of the tropics did actually eliminate many of a fair type. But we have no real right to say that the original dark type in man or his simious ancestors and relations developed more pigment merely on Darwinian lines, that is, by the elimination of the less dark and the perpetuation of those "accidentally" darker, until we know more of the subject. For if human beings became darker why did so many animals become lighter, when there are so many reasons for believing that protective coloration is almost mythical? We do not even know positively if the early negroid was dark or fair to begin with. If he was fair, or white, then the whiteness of the palms of his hands and soles of his feet, since that cannot even by the most enthusiastic be given survival value, must be due to the lack of exposure to the sun. If these races were dark to begin with we can still give no other plausible reason for the palmar and plantar whiteness. There is no explanation here but a forced environmental one, though we can imagine some saying negroes are black because they were originally forest dwellers and found it thus easier to hide from their enemies. To those who regard this as a mere unseemly joke let me commend the fact that Haldane and Huxley (1927), when describing the curious resemblance of the front region of the head in a "lantern bug" (*Laternaria lucifera*) to a small crocodile's head, make the comment that it "has been plausibly suggested that this resemblance is of service to the insect by scaring away small insectivorous birds and mammals." It is hard, indeed, to avoid ornamenting this passage with the objectionable note of

exclamation which makes some foam with indignation. Survival value as a factor in the production of really useless and even absurd handicapping overgrowths has been run into the ground by its advocates. The same may be said about protective coloration and its causes. Must we not, on the whole, conclude that we know little or nothing about pigmentation or its chemistry ?

Dread of the Sun.—There seems no certainty anywhere. For if men grew dark in skin and hair why did so many animals become fair-haired ? Are both colours protective ? Man wants more protection than any other animal in existence, for animals do not as a rule kill and eat their own species as our ancestors undoubtedly did, and as some of our finest contemporaries do now. The antelope and the negro do equally well in the sunlight, though neither likes it in excess and therefore hides from it when it is at its worst. Human beings can usually make some kind of shelter. Social animals when they cannot find it crowd together and put their heads under the shadow of their companions' bodies. This I noted continually with regard to sheep in Australia and in Texas. Sometimes animals and even birds seem to die of sunstroke. But not many so die. There is no real elimination done in this way. Yet when we conclude that the antelopes bleached and that men did just the opposite, we may ask why that nocturnal animal the lion is usually fair-haired, though black or melanoid lions do exist. Has blackness turned out to be an unfavourable mutation ? That is, indeed, hard to believe in mainly nocturnal animals. We may well despair of any explanation if we do not conclude that pigment has no real relation to protection and that given original racial pigmentation potencies some developed through the influence of an age-long environment on one line and another upon a different one. We must come back again to the endocrines and wait for knowledge. Even now we can guess that heat and sunlight affect the

ductless glands, since European Raynaud cases do well in the tropics and would never have been such cases had they been born and stayed there. We might say that the negro in cold climates does not suffer because it is cold but because he is hypothyroidal in our climate. I trust it is clear that what is said here is mainly directed to showing that we know next to nothing of the subject, not that field-naturalists know everything. Let us take Darwin and Lamarck and a few bio-chemists and physicists and endocrinologists into conference and think it out with free minds. All these colour changes have been forced and therefore continuous and not necessarily lethal, and animals do the best they can with such forced changes in the best environment they can find. Truly the weak succumb, but that Lamarck would never have denied. And why Darwin, a true apostle of use and disuse, so ignored Lamarck still remains one of the greatest puzzles in the history of evolution, even if some kind of an explanation of it may be offered.

Man the Hunter.—It seems that there is one aspect of this African coloration problem which has been ignored even by those who might, perhaps, found an argument upon it. Pure Darwinians may be made a present of it for what it is worth. Too much attention may have been given to the lion and the leopard and too little to man. When it is denied on very good evidence that the natural elimination of the darker deer and his substitution by the lighter has been due to nocturnal animals which hunt mainly by scent, it cannot be denied that man, a far greater hunter than the lion and the leopard, has been destroying antelopes for a period measurable rather by hundreds of thousands of years than by mere generations. It is impossible here to discuss the question as to the age of man. But many authorities believe that he was what we should recognize as human a million years ago. To recognize our ancestors as human is to recognize them as

hunters. And not as hunters just beginning to hunt. How long were his semi-human ancestors engaged in the same task ? If it be said that the hunting of food supplies and even of large game has been in progress two million years it may, indeed, be a guess, but it is a possible one. Now for man, and his ancestors, hunting was an employment not only for the night but also for the day. For him alone a general toning in with the environment would be an obstacle. At night a fawn-coloured antelope and a black object are just as visible and carry as much scent for the lion and the leopard. This view has been held by some to imply that the lion's eyes are of little use to him. This is a variation in argument not likely to possess survival value. However little force we may concede to accumulating minute variations, which do not take into account the real orthogenesis of continuing forced changes conditioned by chemic and physical influences, all may agree that, as regards man the hunter, now and in long ages past, any likeness to the environment is, and was, of some advantage to the animal hunted, which knows well that much of its safety lies in being invisible as well as in being speedy. We should be rash if we said this was the case with all animals. The chromatophores of the chameleon are not ruled by the animal's volition. They react through the visual centre and the endocrines, and also directly to the amount of direct and reflected light. That the chameleon is best hidden in an environment " chosen " by the chromatophores is probably a physical accident. It may not even know as much. Many such accidents occur and most are definite reactions to the amount or quality of light. They may be advantageous or not but they are so if the reaction of the skin more or less matches the locality chosen. However advocates of Natural Selection look on this fact it can at least be urged that we have no right to say that these optical and physical reactions arose by accumulations of minute variations or from Mendelian

mutations. Forced environmental "mutations" are not Mendelian according to genetic theory. They are accidents and qualities made use of, as so often suggested. This Darwin admitted might occur, as was elsewhere pointed out. Nor can we say that an animal has become adapted to a safe locality if it has selected it because its colour fits it or if its change of colour is pure physical accident. It seems to me that such views throw at least a little light on many obscure phenomena which we have been taught to look on as adaptations. It is pleasing to note that I am by no means solitary in thinking that adaptation problems are most likely to be solved by turning them upside down. Even more may be gained by turning down the word itself. It is highly probable that, just as philosophical difficulties arise from the obvious defects of an unscientific terminology, many zoological problems have been confused at their source by the doubtful connotations of "adaptation." Different workers define it differently. Some define it as X in a major or minor premiss and produce it as Y in the conclusion. It may be suggested that nothing more calculated to advance biology can be imagined than the appointment of an authoritative committee to define once for all a number of words which by their obscurity do but confound knowledge. We should then be able to see what aspect of the fitness of an animal for its environment was meant. At present the process of adaptation may mean internal volition, external purposive direction, entelechy direction, accidental variation with survival value, forced variation with change of environment, or undiluted Lamarckism. Probably more varieties and variations of zoological thought will occur to others. The word as now used is a disgrace to science.

The Springbok.—It is, however, almost impossible to make some abandon a dogma, even if it leads them logically to absurd conclusions. Let us take the example of the African springbok, which, when alarmed or excited in herd

"pronks" or makes prodigious leaps, sometimes ten feet high, into the air and in doing so displays a light part of its skin. Any definite quality, mark, or custom must, according to the theory, be of advantage and possess "survival value." The use of this habit is, so I have heard, "to keep the herd together." So we must conclude that other antelopes cannot keep together as they do not practise this habit. Now if I were asked to suggest a possible use for what is probably a quasi-hysterical reflex of alarm, I would rather pin my faith and flag to the belief that they jumped into the air in order to see from what quarter the danger came. Let me hasten to say I do not believe this. Otherwise it might soon be stated gravely that it is the true explanation. If this habit has any value at all it is in the possible alarm it may spread in the herd. I shall, however, show later some reasons for believing that its value is practically *nil* and that survival in such cases is due to a cause which is easily comprehensible, and very far, indeed, from being minute or even curious. In the same way we see it declared that the white patch under the rabbit's tail is of definite race survival value by showing young rabbits who do not know where they live how to go home by following their dam's scut. This is one example of the way some naturalists, not always of the "closet," make shots without knowledge. Those accustomed to social animals living in warrens, such as rabbits, or the "prairie dog," the marmot of the North American plains, know well that the young get intimately acquainted with their houses and the near neighbourhood before they go any distance. In the same way bees, during the week or two in which they learn their hive business, take short flights to prepare for the longer distances they must go as they set about the hard last portion of their lives. We often overrate instinct, simply because we cannot for an instant believe an animal can learn and get to know, in any epistomological sense we like, its own

particular business. And thus we attribute to accidents an almost magical survival power. I can myself offer no explanation of the kind for this habit of the springbok, but I think it might be pointed out that distant enemies, including man, may be and actually are instructed as to the presence of these animals by this extraordinary and dangerous habit, or as I prefer to put it, this dangerous involuntary reflex. Surely it is a singular case of adaptation.

Sexual Selection.—Just as Eliot Howard in his work on bird territory showed that an immense amount of the learning displayed on Sexual Selection was as much wasted as the labours of the schoolmen on imaginary and absurd theses, we may learn that the lure of adaptation has led most zoologists far from the truth. Let me note, however, in this place that the view suggested by Wallace and strongly supported by Hudson, that it was the energy of the male and its overflow which gave it beauty and victory combined, preluded Howard's views, which show another aspect of energy not inconsonant with the view of Wallace. To resume, we may say that if, for instance, we examine the pretty theory of warning colours we shall often find ourselves landed in absurdity. Warning colours are one form of adaptation ; they have survival value. Perhaps. And perhaps not. In several books I have noted the theory applied to the conspicuous striping of the skunk. I happen to be well, in some ways too well, acquainted with skunks, and the notion that they need any more protection than they have already is ridiculous. They are even immune to rattlesnake venom. (T. Barbour, 1926.) They are beyond attack and may therefore vary in the most extravagant way if they can. Their build, attitude, lifted tail and curiously stiff and arrogant strut protect them, for not an animal on the prairie but knows them just as well as man does, and is to the full aware of their dreadful defensive apparatus.

When we consider that apparatus we find its origin in glands of a semi-sexual character, that is such glands as notify to each other the presence of the sexes. It is not a constant odour, for I have skinned skunks, killed by strychnine in eggs, which had not expelled their secretion and were not offensive. But they usually emit an odour distinctly recognizable down wind at any time. If it be said that the white stripes make the animal visible at night so that it can be recognized at once in the semi-darkness, we may remember that an animal of prey does not need to carry a white flag, and would obviously be better without it. Personally I dismiss these stripes "with costs," and request a better case from their advocates. It may be noted with interest that T. Barbour remarks on the odd fact that the zorilla of South Africa and the Malayan madaus smell like skunks and have similar markings. There may be a pseudo-endocrinal connection between the odoriferous glands and the coloration.

Zoology and Pathology.—It is with some satisfaction that I observe how often those who seem purely wedded to the orthodox views do all the same fall from their faith. In a very charming book W. P. Pycraft with some delicacy does seem to admit that colour may be due, or at least its changes may be due, to food, relative humidity and so on, that is, to the environment which is properly taboo to the Wahabis of Neo-Darwinism. I trust this will not be taken as offensive. There is always something admirable, if not terrible as well, in a stern unbending scientific Tory. Worse than having to admit that the environment may have some influence, is to have to give details, while ignoring their bearing, which show that fate and the general nature of things may deprive animals of vast advantages they once possessed. For instance, what can be more unfortunate for those poor little fish which begin with beautiful translucent bodies than to find, we may feel sure to their personal and scientific discomfort, that they are acquiring

by minute spontaneous variations, or by subtle mutations due to the gymnastics of the gene, or by the degrading influence of their environment, characteristics which make them more and more visible to their enemies, which but lately passed them by with closed jaws and never so much as a gesture of hostility?

Is it not, on cool consideration, fairly certain that any colour scheme does not come into existence as protection or warning but that it arises from physical or organic conditions of which the animal concerned may draw all the advantages its nervous reactions, or "mind" allow? For if we inquire into quite another means of warning we shall see that neither genetic variations nor Darwinian minute variations can have played any part in the construction of the rattles of *Crotalus horridus*, the common rattlesnake or "rattler" of North America. All snakes cast their skins, but so far as I know none but this species has suffered during its early evolution from the obviously pathological condition which disabled it from getting rid of the dead skin on the terminal segment of its body, to which the rattle owes its origin, although the terminal segments of the bushmaster and some other snakes are hard and spiny. I am aware of the intense reluctance to admit them shown by the few zoologists and physiologists who have ever heard of my own suggestions relative to the rôle played in evolution by pathological states, due either to stress or to some unknown nocuous or disturbing factor. This reluctance is, as I think, happily not shared by every investigator, and if to support me I quote G. W. Nicholson on the strange and remarkable neglect, shown by almost every biologist, of the vast experimental school of pathological experiments, it is possible that this number may be minutely varied if by the addition of only a few open to new environmental influence.

"But I would go further and blame biologists much more severely for their entire ignorance of and contempt for

pathology. We pathologists can dispense largely with experiments, since nature has done them for us on a grander scale than is possible for the boldest experimentalist. We have unbounded opportunities for studying the effects on the body of every conceivable alteration of the environment. So long as the biologists ignore us completely they will have but a very imperfect idea of the adaptability and powers of reaction of the living organism." (Nicholson, 1926.) I may be excused for saying that my own views on this subject, expressed with some completeness in 1920 and 1926, may be held to corroborate this.

The Rattlesnake.—It is difficult to see how any can deny that the obviously abnormal retention of the dead dry skin of the rattlesnake's terminal segment is so far from the physiological course as to be rightly described as pathological. Moreover, such a new appendage in its primal form, although obviously not minute and as obviously not genetic unless we describe a failure to denude dead skin as a mutation, can have been at first of no advantage whatever to the species in which it occurred. It is nowhere denied that an accidental variation may be improved on by further variations of the same kind. But this has no relevance to Darwinism, whether new or old, until the result becomes capable of being used to some purpose. Outside of those who manage to believe in a kind of orthogenesis which implies something foreseen, few or none can imagine that even two or three years' additions to the early rattle were "meant" to be useful, nor can we by any strain imagine them useful until they made a considerable noise. This might be a real improvement, for then it would become a help as a warning and as a sex call, which I believe to be one of its chief uses. Otherwise, why, in a perfectly quiet part of British Columbia, where there were neither horses nor cattle about, should I have heard these calls continuously going on one night when I camped alone? As soon as it was thus useful we may say it was of some

survival value, but it must take an uncommon amount of faith even in the most orthodox to believe that it was of the remotest value on its inception. It may be admitted that it is now of real service in scaring cattle and horses. It is said by some experimentalists that the rattlesnake's organs of hearing are such that it cannot hear the sound it makes. Those in any way acquainted with the theory of music and the mixed and mingled vibrations of any "single" note will hardly be convinced that this is true. Whatever a snake's mentality may be we can scarcely decide, but that it is aware of sound and its effects we cannot doubt. The problem may be left to those who think it can be solved on some accepted principles. The view that it is an accidental continuously inherited malformation is not likely to appeal to the Darwinian, or even to the Lamarckian, who has not studied pathology as an essential department of general biological science. The utterly unfounded belief that abnormalities are due to some factor so extreme in its nature as to make them of no real interest seems too deeply implanted in most biologists to be easily uprooted. But our most savage certainties are usually rooted in ignorance which has never met with contradiction.

Herds and Evolution.—Before finishing here with a subject which surely can never be wholly done with, I may return for a moment to the subject of herds and what they may mean in evolution when looked at from one point of view. Let us take again the springbok of South Africa and the once even vaster herds of the North American antelope or prong-buck. Owing to the white man and his firearms these herds are practically things of the past. For every day the springbok gets rarer and where millions of the American antelope used to be seen they are now hard to find. We have been told that the coloration of both these animals is protective and for many years we all believed it simply because we were told so and knew no better and did not

think. Yet how came it about that they throve so mightily? Was it truly because they gradually grew less and less visible to their enemies? They lack all means of defence. They cannot ring round their young like cattle, nor does the springbok possess any valuable weapons but its speed against any enemy. The American antelope is in like case. Here I speak of individuals. Do they, or did they before the white man came, possess any sort of herd and social defence against extinction? It is obvious that they did. For their immense fertility and the vast ranges over which they roamed bred them in such numbers that lions, leopards, cougars and jaguars with their vastly less powers of reproduction were unable to make any real impression upon them. Why in such cases should they need to vary towards any form of protective coloration? Not long before my own time in the United States antelopes used to migrate in such vast herds through parts of Wyoming and Montana that their armies took days to pass any given point. What did it matter to the race if the comparatively rare cougar, though it always increases as possible food increases, took toll of such a caravan? Individually weak, they were as a race invulnerable by any common or recognized enemy. We begin to learn now through the labours of those who are the pioneers of that fascinating branch of biological science which we call ecology that such increases in fertile animals are common and also rhythmic. In the end their greatest enemy appears to be their own enormous fertility (Hudson, 1891). They suffer from disorders which always seem to attack over-populated areas, and are swept away by epidemics which force the remaining members of the herd to travel. As they seek new and healthy pastures the cougars and bears which almost always affect particular territories again tend to diminish with the diminished food supply. Thus a kind of balance is usually established. Have we any reason to say that the colour of the antelope is of any great use to it? I can conceive of one use. They can

stand heat and sun and thirst and hardship better than those who yet retain the colour and natural physiological conditions of body with which their forestal ancestors seem to have been endowed. If that is so we understand something of the reason that their young are so often striped and spotted. And in taking that view may we not ask how long a reed-buck or water-buck would survive if either were forced at once to gain a living under the glare of a shadeless African day upon the red and yellow veldt where water holes are scanty and far apart ? Surely if Natural Selection has played a part in their drama its factors were not living enemies that such animals would hide from but the stresses of the physical environment. It is, I fear, a concession to the Darwinian enthusiast in protective coloration which will meet with small gratitude. The colour, or want of colour, in these animals will finally be traced to an endocrine constitution varying under the stresses of light, heat, nutrition and other environmental conditions.

Real and False Protection.—It need not, however, be considered that such conclusions definitely deny real cases of protective coloration. Many insects and animals are so coloured. But the early stages of such colour schemes could have been of no advantage, as was shown in the case of the Vespiform moths. We are thus left to ask how and why these early stages occurred. Greenness, say, does not arise from accident, unless it is environmental accident. Or from its foreseen survival value. The day of purely accidental variation is over and the inquiry is now into antecedents. Mutations have never been shown to occur except in organisms in stages of wide variation which can usually be referred to new environmental conditions, abundant nutrition and surplus energy. Experimental conditions are always unnatural : it is therefore not strange that they should initiate variations unlikely to occur or to be perpetuated in the common fairly static conditions of free life. It is, perhaps, not a little curious to observe that

some of the strongest advocates of protective coloration seem always to ignore those cases in which protection would be useful and is altogether wanting, and other cases in which it is present and is yet wholly useless. It should be a pleasant task for any young student to examine such a charming book as W. P. Pycraft's *Camouflage in Nature* to which I referred just now, with the view of discriminating between real and pseudo-protection. How far such inquiry, when pushed, would lead him it is hard to say. He might come to the conclusion that the environment is more powerful than he had ever been taught. In the end he might even speculate as to whether the reversion of some young animals to a striped or spotted ancestral condition was not due to an intra-uterine environment. In any case he should learn to doubt authority and to think for himself. It is by trying out speculative notions, usually ridiculed by static minds even when suggested by experiment or observation, that the organic body of explanatory thought, which we call the theory of evolution, really advances.

Truth and Variation.—Considering this chapter as an "organism," that is, as a little partially closed system, it must be admitted that it shows signs of variation, however unimportant, from recognized theory. How far these will be advantageous is indeed difficult to say. But I should like to commend to the younger students of zoological philosophy the problem as to how these variations arose. Were they karyokinetic or Mendelian or semi-pathological owing to the writer's discomfort in Neo-Darwinian territory? Such questions must seem to many merely ribald. But we shall make little real advance in the theory of evolution until we recognize that the laws of evolutionary construction apply as much to the growth of opinion and theory as they do to what we more commonly call true organisms. It is by forced variation we adapt ourselves to truth.

Looking back upon this section I am dimly reminded of many ancient prospects, such as those found among the

cañons and arroyos of Arizona, scenes of confusion that seem beyond the cartographer or any guide. In that landscape are the bones of dead theorists, and many tumuli, so many indeed that it is hard not to perceive that there waits for me, as well as for others, some obscure place of burial, or at least a spot whereon my remains may bleach. It is fabled in such countries that strange gods dwell in the peaks and in the wilder spaces. If I therefore imagine that on one peak I can discern the very spirit of Darwin and on another that of Lamarck, it will not seem so strange. Here is a great wilderness and it may be that in time to come less rash explorers and pilgrims will tame and make habitable the very places in which their forerunners lay down in exhaustion to sleep among dry bones.

CHAPTER SEVEN

THE HISTOLOGIST AND EVOLUTION

Morbid Evolution—Constructional Immunization—Histology and Evolution—Kidney Development—Evolutionary Operations—Evolutionary Scars—Evolutionary "Tumours"—Phylogenetic Atrophy.

Morbid Evolution.—It has been made obvious in the preceding pages that the doctrines advocated imply that the course of organic evolution has not been a purely physiological but rather a pathological process. It has been taken for granted that an absolutely healthy condition of any given organism, or even perfect function during any long period, is no more than a fiction of the laboratory and the textbook. This is in fact accepted by physiologists. But with regard to these pathological conditions it is clear that the amount of disorder implied is not lethal or of a kind which prevents the perpetuation of the individuals or groups concerned. It may indeed be little disturbing to the evolving organism as a whole. Physicians know that many diseases which in the end are fatal may be disclosed by no obvious pathognomonic symptoms till the close of the secret drama. The human body is so tolerant of a certain amount of malaise as to induce many to think that some discomfort is normal. It is hard to say that they are wrong when it is recognized that the ordinary processes of metabolism are perpetually being disorganized by the stresses of life. So much as regards the part played by pathological conditions can easily be accepted, but the thesis here to be explained and defended implies that a continuous environment of stresses on groups, stresses probably affecting both sexes

alike, in the end so affects the race through the upset of function as to induce changes of structure enabling the altered function to continue on the borderland of health, that is to say, without obvious morbid or lethal symptoms. Such a view obviously negates pure Darwinian variations of unknown origin as the main factors in the development of the soma. That Mendelian mutations which stand without any assigned cause for their appearance were responsible for the evolution of the main organs of any soma is also negatived. Nothing in the shape of accidental additions as the result of genes or genetic factors can reasonably account for the obvious way in which structure follows function. Few attempts, indeed, have been made by geneticists to show that gross anatomical structures were built on these lines. What uncommitted anatomist would easily accept the view that the architecture of the head of the femur was due to anything but hereditary function, following upon stresses and altered, generation after generation, by the forced altered behaviour of the osteoblasts? It might seem that geneticists must account for the changing inherited reflexes of the osteoblasts by postulating genes for behaviour and those complex cellular reflexes which in still more complicated cases we can, with Pavlov's full approval, call instincts. It seems fairly clear, however little we know of inherited functional mechanism, that it operates through the altered reflexes of all building cells under the influence of altered catalysts and hormones, those which operate and arise in such cells, and those of neighbouring tissues which influence them. Both alike, or all, spring from the biochemical action of the inherited catalysts, which are protoplasmic tools with all the possible potencies of tools, not parts carrying any definite character or variation. In other work I have attempted to show that such a conception, when combined with breakdown under stress and following repair, helps us to understand the strange complexity of the heart and other organs, the continuous growth

of muscle and the diversion of ancient evolutionary construction into other functions such as is seen in the embryological history of the Wolffian Body and ducts and in numberless other cases. Much of this conception or theory was reached inductively by observing, not merely in the body but also in all human work with which I had any acquaintance, that structure rose out of function and that excessive function led to reparatory variations. This no intelligent engineer, architect, or builder could deny. A little consideration enabled me to see that in rules of law and custom a similar mechanism of construction was at work. Breakdown and repair was in fact a real general principle or Law, and from it many legitimate deductions might be made. After referring to the other work mentioned there is little necessity here to say more in favour of this study of morbid structure and function, where a partial breakdown is seen repaired in following generations, or to give more examples.

Constructional Immunization.—And yet I think that the argument that pathological states play a great part in evolution may be somewhat strengthened if we consider one aspect of the life of organisms which, so far as my reading goes, appears to have been taken little account of, although it may be said that physicians recognize it, if not in theory at least in practice. I refer to the undoubted fact that not only is the body perpetually growing older, but that it is in many of its most important constituents, dying daily and hourly. Millions of erythrocytes perish and their dead " bodies " have to be swept up and swept out of the system. So it is with the many varieties of the white cells. They may die in billions in an abscess but every moment untold numbers of them die a normal death. The same can be said for all connective tissue and epithelial cells. The body as a whole has to meet the demands made on it by the death of its individual members. To what extent the actual growth of the spleen is due to its functions

as a mortuary and perhaps execution ground for sick and dying blood corpuscles cannot be stated, though the actual facts may suggest the hypothesis. One of the factors in many diseases and disorders is undoubtedly a vastly increased cellular death-rate, which has to be met not only by the restoratory action of hæmatopœic tissue but by somatic sanitation and immunization. In some streptococcal septicæmias all the white cells disappear. The patient has to endure not only alien toxins but the death toxins of its very defenders. If this is so, even in what we call health, how can we deny that death and disease, expressed in terms of cellular senility, must be factors in evolution? For these observations must apply *in utero*, if not from the time of the very fertilization of the ovum at least from the gestational era when the fœtus has its own differentiated tissues and its own blood-stream. Life is thus a fight against death from the beginning, and when considering immunization generally, it is hard to believe that during evolution these factors, which cannot be called germinal or cytoplasmic, have not had constructional immunizing results.

Histology and Evolution.—This view, combined with what was said before, implies that pathology should make part of the armament of the biologist. Here, however, we may go beyond general pathology and endeavour to enlist even morbid histology as a witness. That the apparently obscure work of the histologist should deserve much attention by biologists is hardly likely to be at once accepted. The arguments drawn from the descent of the testes, or the evolution of the uterus, are at least easy to understand, but the suggestion that congenital aplasia of a kidney or the production of three functioning kidneys may have some pertinence to biological questions is likely to be received with impatience. Yet if a study of such errors of development, clearly akin to pathological states, strengthens the case for what can be called pure epigenesis, by which I mean the view that every new development depends on the sum

of all the factors of the whole internal and external environment at the time, and not on introduced genetic mutants which are guess-work, it may help to disperse the still powerful ghosts of preformationism. Let me put it this way. A kidney is not a kidney because its protophase was forced to be one, or to become one, because of definite germ factors, but because, owing to a succession of chemic and physiological and physical antecedents not interfered with by any intrusion, or omission, or time and space relation change of those antecedents, it was and is so built up through a series of forced and often morbid changes as it fulfilled its increasing function. In morbid histology we note how any known factor can alter development and of logical necessity we assume that any unknown factor can do the same. We are therefore compelled to take into account for every new kidney all the factors of the environment, and when we note the way errors arise we are forced to believe that at any given period of nephritic evolution the sum of all such factors was at work thus preparing for the following stages. Such growth changes might in any one individual be exceedingly small, but the result would add up in succeeding generations if the environment remained stable or so nearly stable as not to overcome the natural inertia of a development "in motion." It is not only in the phenomena of heterogonic differential ratios that we can see that a growth once begun tends to continue, just as an organ can degrade if it is disused. It may even continue till it destroys balance, either the balance of the animal's body, or its balance with its environment. This is fairly in accord with Lamarck's prime factor in development, which was not the influence of the environment but the tendency of organisms towards increasing complexity. The tendency to increase in size by conflicting and immunizing action in construction, which I developed in *Malignancy and Evolution*, may be looked on as a rider to this theorem. These tendencies when wrongly interpreted are very apt

to give rise to orthogenetic heresies. But putting all that aside, let me say with a very notable student, " heredity is cumulative environment."

Kidney Development.—There are certain facts connected with the development of the kidney, known only to special students of histology and histogeny, which are pertinent to these questions. I have not so far come across them in any textbook of embryology in common use. I extract one set of phenomena to begin with from the work of G. W. Nicholson (1927). " As soon as the ureteric bud is formed, it is applied on its inner side to a mass of mesoderm cells. This is the metanephrogenic blastoma, which corresponds with and forms the continuation of the nephrogenic cord, from the more cranially placed regions of which the pro- and meso-nephros are developed. . . . The metanephrogenic tissue soon forms a cap for the ampulla of the ureter. It ascends with the ureter, and is quickly isolated from the surrounding mesoderm. When the primary collecting tubules are formed it is stretched by them and torn into a corresponding number of pieces, which again form caps for them. The tearing is repeated as collecting tubules of higher orders are produced, the edges of the caps coming into apposition and uniting as the result of the proliferation of their cells."

Evolutionary Operations.—For what purpose have I quoted this early section of an intensely interesting paper? Those who have followed the general argument will see at once that from the point of view of the theoretic biologist the most striking part of the picture is that which shows the tissue of the metanephros forming a cap for the ampulla and the ureter. For after that we get an " evolutionary operation." The cap is split away from its parent mesoderm. Are we to ask Mendel or the genes for help here? Is this, a most important development process, an " advantageous mutation "? But, as we see, it is only the preliminary for a whole set of what I shall call evolutionary

pathological steps in kidney development. When the primary collecting tubules have formed they tear this avulsed cap into a corresponding number of pieces, which again form tubule caps. These " operations " continue and then we recognize what Keith so finely calls " evolutionary wounds " in the very act of healing. For the torn edges of the new caps proliferate and, coming into apposition, unite. Such evolutionary wounds are part of the actual machinery of evolution, and the very phrase by itself implies repair as one of its great factors.

Evolutionary Scars.—I am by no means ready to affirm that the writer of this paper will agree with me in all the conclusions that can be drawn from this, although I regard him as a pure epigeneticist. He has avowed his disbelief in genes. It was not Nicholson's purpose to do more as regards the philosophical side of this paper than to record, at the end of it, his dissent from any form of preformation. But we may use his description of facts as we will, and again it may be asked how any can continue to deny that definite tissue disasters occur in evolution and are healed? We must not say, as these tissues are healed, that they were *meant* to be healed. We are by no means forced to conclude that if these ruptures had not been healed in the way we see them that there could have been no such function as urine secretion. It is only highly differentiated tissues which cannot as such change their path, and they, as we learn from other sides of biology, may even go back some way in de-differentiation and take other possible forms. I see no reason why the mammalian kidney might not have been what we should now look upon as a " tumour," perhaps a mass of lowly differentiated tubules, which together could perform the main nephritic function if union still took place between the embryonic metanephrogenic part with the ureteric part, which leads the way. That is to say, it is the ureter on which further differentiation depends. Once we acknowledge the possibility of such tissue rupture and

breakdowns as a possible step in evolution we are introduced to a new and powerful cause of variations, which can have no relation whatever to genes or mutations. To use genes is merely to state that that has occurred which we know has occurred, and no more. Let me note as regards these evolutionary wounds that they heal without a proper scar. We may find lines of former division as we do in the *septum scroti*, or in the mid line of the jaws, or, as I think may be suggested, in that line which is seen as a fine strand of tissue leading in the third month of fœtal life through the yet unbroken abdominal wall, and serving as an evolutionary guide for the gubernacular outgrowth. I refrain from remarking on this curious strand as evidence for Lamarck. (A. Keith, 1921.) In late embryonic life, however, proper scarring with granulation tissue may be found in the kidney. This suggests that at the stage of the evolutionary rupture of the metanephrogenic caps there was then true ordinary healing as with any late injury. That these stages were simplified in phylogeny, thus usually healing in definite uninterrupted sequence in ontogeny, and taking on physiological characters, is certain. I should find it difficult to believe that the elephant's pulmonary adhesions did not arise in something very like a true pleurisy, or that the same could not be said of the adherent avian lungs. We may compare the fixation of parts of the gut.

Evolutionary "Tumours."—In *Malignancy and Evolution* a hypothesis was suggested to account for those glands, originating about primitive nephridia, which we now call ductless glands. This hypothesis was founded on the basal supposition that the nephridia concerned, as their activity in function failed when the advancing true nephritic or kidney system gradually took over their functions, became, if I may put it so, victims of their own poisonous secretions which had to be immunized and used by the organism. The resulting tissue reactions resulted in a new "organ," not yet an organ. It was an evolutionary tumour "on trial."

Obviously such a scheme lays its advocate open to the charge of being too imaginative. But after long consideration I am unable to see any other possible or reasonable mechanism for the origin of these glands. That the kidney system has no embryological relationship with these ductless glands we know, but it is not the mere possession of an enlarged and powerful duct apparatus which distinguished it from them. It can even be suggested that the course of nephritic development supports this view of the ductless glands. About the primitive ureter which is formed from the Wolffian duct grew up the early metanephrogenic blastema, from which the secretory part of the kidney arose. Why did it arise at all? Can it not be suggested that here we have a mesodermal structure which was also a tissue protective reaction to the primitively inefficient drainage system? If so, we can begin to see again how secretions and excretions, all toxic as regards their place of origin if not carried away, and all still toxic or aggressively stimulating as regards the soma, or parts of it, give rise to permanent hereditary structures through forced function. However speculative this may seem I commend it for consideration by those capable of forming a judgment. And I venture to add that any biologist who recognizes his own incapacity to deal with these and similar questions has no right to sit in judgment upon Lamarck or Buffon or Erasmus Darwin. That many biologists are incapable in this way histologists and embryologists know and resent. If I agree with them in that respect it by no means follows that they will agree with my own hypothesis. But if not, they will at least be able to give intelligent reasons for turning it down. Perhaps some, however, will see the possible conclusions to be drawn from a passage from Bromann, quoted by Nicholson, which I shall compress into a few lines. When metanephrogenic tissue is wanting no tubules or Malpighian corpuscles can be formed. The protophase of the ureter can develop, but sends out no branches. If

the Wolffian duct produces no ureteric bud, then no ureter, no pelvis and no collecting tubules are formed. With the metanephrogenic blastema undeveloped there can be no kidney on the side affected. It may be added that in such cases there is a kidney on the other side double the normal size. Is this hereditary function or preformation in disguise?

Phylogenetic Atrophy.—Nicholson notes, without drawing any of my conclusions, that large parts of the metanephros undergo degeneration in ontogenesis and disappear without leaving a trace. He says, rightly of course, that this atrophy is physiological. It is now but was it always so, if we consider atrophy, properly speaking, as pathological? I do not doubt that these parts atrophied from disuse, probably because the blastema, in early inefficiency of function, grew up beyond later necessity. As Nicholson remarks, any complete absorption is rare in embryonic life, so that we are forced to conclude that this metanephric atrophy "without trace" took place in phylogeny. If nothing here seems quite convincing I am certain that these points will bear thinking about and that the conclusions drawn are strictly relevant to the main questions dealt with in this book.

CHAPTER EIGHT

Preformationism

Weismann and the Gene—Thomas Huxley not a Darwinian—Use-Inheritance and the Brain—Latency in the Germ-plasm—Genetic Contradictions—Preformation and Mechanism of Change—Gene Hypothesis and Huxley—Theory and Experiment—Organisms and New Creations—Preformation and Chemo-differentiation—J. S. Huxley and the Human Foot—Passing Simian Characteristics—Muscle Deployment.

Weismann and the Gene.—Many biologists who are still young well remember when the germ-plasm was held to have settled nearly everything and Weismann was regarded as a biologic god who had almost replaced Darwin. For by then it was clear that Darwin had not said the last word on the machinery of evolution, as many had previously held. So some workers retreated into a newer kind of creative mysticism and, bidding Mendel shake hands with Weismann, made mutations and genes take the place of ids and determinants. To many old Darwinians the *id* had always looked not unlike what the old Schoolmen called a quiddity. It was a something, or a "somethingness," and the gene with a visible place in a chromosome appeared a trifle less mysterious, especially as it could actually do something, though what it did was, as others thought, merely to shift inessential and probably fleeting characters. But still the gene did not satisfy everyone. The orthodox felt that it did not and, being not unwilling to make a harmless concession, they allowed it to be called a factor. A gene is a generator and very vague after all. But the essence of a factor is,

so to speak, factorial: it "makes" something, it is active. But, even so, the authorities on the gene were themselves a little uncertain as to its power and the limits of its powers. At last, and lately, even T. H. Morgan admitted, probably with some natural reluctance, that any given gene not only did the work assigned to it as its proper and peculiar task but also influenced other parts and perhaps the whole organism. So far very good indeed, for if the gene was a general agent and a factor it began to look remarkably like a potentiality. It seems that this is how the matter now stands.

Thomas Huxley not a Darwinian.—It would, however, be to underrate the candour of the human race if it was imagined that the stricter doctrine of the gene will soon be openly disavowed. Like the id it has to crumble. It is crumbling daily. For if the gene has general as well as special work how could it be what it was said to be? So function again began to come into its own. Function is now really in the ascendant, even if its mechanism is due to the work of factors. Perhaps the cytoplasm had some real work to do. Perhaps the organism as a whole had effects not attributable to genes and factors. Perhaps it changed heredity factors by function. And then assuredly Lamarck could hold up his head. But though function does seem to be again recognized, it is still often combined with some undefined general tendency for an organism to come out upon definite adaptional lines in some way laid down very long ago. Thus to many workers it seems incredible that function forced by the environment upon protoplasm, which the whole of zoology shows to possess infinite potentialities, should build any complex organism without the assistance of an agent which predisposed it to work out as it has done. This tendency to explain adaptation and co-ordination as the result of innate purposiveness is, of course, contrary to Darwin's views of accidental accretions of small variations, but it is strictly in accord with the views

of Thomas Huxley, another proof of the obvious fact that Huxley was never really a Darwinian. Probably it would not have been necessary to deal with this Huxleian conception if it had not been supported, a very short time ago, by Sir Arthur Keith, whose great authority is hard to resist. Huxley's attitude is most plainly seen in his lecture on the Hand. For Huxley believed in some form of teleology not, we must suppose, due to any deity. He tried to reconcile it with evolution by stating that the human hand was "sketched out—that the purpose was foreshadowed in the molecular arrangements out of which the animals have come." This implies that, with the hand, all other animal forms, and we must naturally add plant forms, were equally "sketched out" in the molecules of the earliest protoplasm. If so why should we not conclude that they were, even more roughly let us say, sketched out in the molecules of the heated gases from which the earth arose, and further that they lay dormant in the electronic constitution of matter? No pure logician or mathematician without biological learning could interpret these doctrines as meaning more than that all life was potential or that some unknown power had ordained by intelligence all that was to happen. It might be argued, doubtless, that this intelligence was not "outside" but "inside" matter, that it was a directing principle. But such a principle is an entelechy, such as that favoured by Driesch, which really means nothing but an attempt to avoid the conclusion that matter and protoplasm simply had to do just what they were forced to do by physical and mechanical causes or the internal and external environment.

Use-inheritance.—Huxley utterly rejected the evidence in favour of "use-inheritance" as stated by Lamarck and Darwin. It is obvious, indeed, that their views on this point were not established. They were, and to some still are, a matter of doubt. But in this matter Huxley

brought no convincing evidence. His rejection is not the proof that some consider it. Obviously there are other possible solutions than "purpose foreshadowed." However lacking in proof the pure Darwinian views, they can, as we know, be very strongly held, and it is far easier to conceive that evolution actually did occur through karyokinetic "accidental" variations accumulating when advantageous than to believe for an instant that they were "inherent" in matter. The Neo-Darwinians at least mean something which does not nullify scientific method by a statement which cannot be proved, and if proved would still leave us seeking for an explanation of the stages of evolution. As much can scarcely be said for the Neo-Mendelians at their present stage of evolution. For by explanation I mean a formal statement of each transformation so arranged that the transition from one to the other arouses no obvious logical difficulty by employing assumptions incapable of analysis. When we have such a statement on functionally successive lines no more is needed. Those who work upon function and yet find it inadequate may resort to some such view as Huxley's, of which there are many varieties, or come at last to see that use-inheritance is the only logical way out of the difficulty. That we do not yet know definitely how it occurs is nothing in face of the evidence for it. Harvey did not give up his conclusions as to the circulation because in his time he was not able to trace the final distal passage from the arteries to the veins.

Use-inheritance and the Brain.—Thus if acceptance of a postulated but still unproved series of events leads to a reasonable explanation of a problem while its denial throws us back among equally unproved but far more unlikely assumptions than those postulated in the former case, is it not in accordance with the generally accepted methods of dealing with evidence to take the more reasonable explanation and to reject the other? But in

Concerning Man's Origin (1927), Keith writes: "If man by using his brain could make that of his child a bigger and better instrument, then there would be within our reach a sure way of improving the human race. Were our simian ancestors a race of brain users? Did they become possessed of skilled and shapely hands by manual training repeated through endless generations? If the effects of use were hereditary, then the explanation would be easy." Though I accept the view that this actually is the one explanation possible, it would be preferable to say that the child in question would not necessarily possess a bigger and better brain than its parents, but merely that, on the continued brain exercise indicated, he would tend to increase its heritable cerebral power and activity by a slow but profoundly complex series of hereditary nutritional and endocrine changes diverting energy towards brain enlargement and development of the cells and their dendrites, so long as he was not thrown back by influences derived from a less intelligent mother. We need say nothing here of the obscure tendency of a started growth to increase until it is positively stayed. As regards the hand we may say much the same, though here we have a most interesting result of the denial of manual specialization brought about by early man's infinitely varied employment of it and its definite escape from being used as a foot, owing to the upright posture, which is largely, if not entirely, derived from our early ancestors' arboreal experience. This is, I take it, one of the reasons for its preservation in its semi-simian form. Thus the retention in many cases of fœtal forms by man has no other significance than that their abolition in other primates is due to their being supplanted by some development in excess of those reached by man.

Latency in the Germ-plasm.—It seems that Keith accepts Huxley's views of use-inheritance. He quotes him without criticism, and his remark that Huxley in 1876 was

then "at the zenith of his intellectual power" seems to imply approval of the statement that the human hand was "sketched out—that the purpose was foreshadowed in the molecular arrangements out of which the animals have come." In another place Huxley spoke of a "predetermined line of modification." "Thus," says Keith, "Huxley had come to the conclusion that the starting or controlling forces, which in due season were to give man his hand, lay latent in the germ-plasm of that simian stock which ultimately blossomed into human and anthropoid shape." And this he seems to approve.

Huxley and the Hand.—We owe much to Thomas Huxley. He fought a hard fight on behalf of the evolution theory and brought about victory earlier than it would otherwise have been reached. But we all get rid of our prepossessions with enormous difficulty. I cannot see that Huxley ever really freed himself from his own past or from Cuvier's conception of archetypes, though he repudiated the use of the word as more than a convenient diagram (1853). This must be the real explanation of his attitude. But the very conception is clearly of a semi-mystical nature. Thiselton Dyer long ago reached similar conclusions. He looked on Huxley as by no means a sound Darwinian. Huxley, in fact, wrote that "evolution might conceivably have taken place without the development of groups possessing the characters of species." (Essays, *v.* p. 41.) With Huxley's apparent distrust of much of Darwin's machinery none need quarrel. Perhaps his one notable pronouncement in theory was that evolution could be looked on as a generalization of facts embryological and palæontological. That palæontologists are usually, or at least very frequently, Lamarckian is therefore a significant fact. That Huxley, towards the end of his life, turned towards latency, that shadow of the archetype, is not so surprising. Real epigenesis as, say, a histologist might see it, was something he could not swallow, and

having rejected an external and an immanent deity he made a deity of the "nature of things." His reputation being what it was he was bound to have followers. It is with reluctance that I commit myself to the statement that he was in no way a great original thinker in spite of his many additions to morphology. A classifier and a teacher, yes; a wonderful advocate with a case put into his hands, yes again; but I ask anyone to name any really great or fresh addition made by him to the theory of evolution except this theory of the hand, which seems to mean so little when analysed.

Genetic Contradictions.—How then can it be held, at least as a temporary shelter from true epigenetic descent with modifications, by those who have in numberless ways shown themselves capable of brilliant original thought? Greater men than Huxley have held theoretic opinions which every piece of their practical work undermined. We may note that many of those working experimentally with tissues and with the less highly organized animals in which repair and regeneration are easy to be seen, are yet able to hold the opinion that the environment is nothing and the nature of the organism everything. They and the environment they use do miracles and yet they proclaim themselves the Slaves of the Gene. They destroy and re-create, pull down and build anew, and yet, as they mix and alter saline and nutrient media which change all action, say that nature does not work as they do. If they did but cast their eyes abroad and look into the operating theatre of nature and into her pathological laboratory, observing death and decay as well as the failure that does not carry death but repair, they would surely have some doubts. It may be that nothing but pathology in its wider aspects could achieve the task of overthrowing their confidence.

Preformation and Mechanism of Change.—How shall we account for those who accept this view of Huxley's while still insisting on the necessity of supplying a definite

mechanical apparatus for each new step in evolution? If the hand was "sketched out," was the machinery also sketched out or foreshadowed? If it was not, but is to be looked on as a complex set of accidental determinants, where do we stand? There seems to be no answer. We cannot have a remote general cause determining in its right outline what we call a hand and then demand an *ad hoc* set of finishing-off causes. For, if the simian stock contained the "sketch," in what stock was the simian fore-paw latent? And what were the determining finishing-off causes which completed it in the simian enough for it to be a sketch of the human hand? Can we not carry this back to the primitive ancestral amœba? Assuredly we can, and we are then faced with the certainty that in this single cell either all living forms were "sketched out" or that we are in truth dealing with nothing but potentialities.

Gene Hypothesis and Huxley.—This seems far too important a matter to be settled by any authority, however high. Those who have considered the remarks made on the gene and the place it occupies in so much modern biology will note the strong likeness between this theory of Huxley's and that which underlies the gene hypothesis. For sketched out or not, every possible "character" is given by genes its actual material place in the chromosomes. Those who accept it will not admit that a new gene can be picked up from the environment. In an early edition of *Mendelism* Punnett told us genes can be lost but cannot be acquired. This implied that evolution may be largely determined by the loss of factors. This statement has disappeared in the 7th Edition. Bateson told us all things new lay in the germ and were unpacked. Here I suspect what I have called, too disrespectfully perhaps, the "molecular bag." But why should we stop at the molecule, when we have the atom and the electron waiting for employment? Is this the pass to which modern Darwinism is reduced? It is idle to talk of machinery or to lay stress

on function when at the basis of all things lies a metaphysical predestination which cannot be distinguished from non-epigenetic preformationism, and again I turn to Starling and ask for " a new creation " each time in a new environment, and to Lamarck for at least some of the causes of change. Pathologists, chemists, and physicists can give others, and in the end, perhaps, all we are ever likely to know.

Theory and Experiment.—After such a note on what is more or less pure theory it may be a relief to turn to experiment. So far only the slightest reference has been made to the minuter and more obscure processes of somatic construction. But as the general trend of all that has been said leads to a denial of anything like design, or to the interpolation of " mind " in any form, if by it is meant anything but a pure figure of speech, it seems necessary to touch at least the borderland between pure physiology and bio-chemics, chemistry and physics. To mention the four together shows at once the impossibility of anyone dealing with the subject adequately. But it is clear that the phrase " mechanico-physical " means the orderly and inevitable passage of one organic stage into another, so long as the various physical and chemical developments are not interfered with by some internal or external interruption of rhythm. We see this in the passage of one purely chemical state into another, as when acid and a metal produce a metallic salt. Such a process may be hastened by heat, which acts as a catalyst, or by some recognizable agent such as boron in accelerating the fusing of some metal in a crucible. Purely physical changes are inevitable: they cannot be interfered with except by purely physical causes. As potential runs down we tend to get these more complex and less energetically inevitable processes which we call chemical. It is necessary to take into consideration the nature, or what we can learn of the nature, of energy. Although immense physical energy

and even minute areas of intense action may and must be found in any organism, it is obvious that any vast expression of primal energy in an organism would destroy it. Even minor, but abnormal, exhibitions of molecular forces can do as much. Organisms are built up upon a path selected by trial and error and can only avoid purely physical dangers by keeping to that continually narrowing path. It is not unfrequently stated that blood-clotting in certain conditions is an "adaptation." This is a wrong way of looking at inevitably acting physical and chemical and colloidal factors. We see that at any given stage of analysis we may call these reactions one or the other, or the third. The point is that some warm-blooded animals with a high blood-pressure which, as experience shows, are continually liable to wounds implying loss of blood, could not have developed if, as loss of blood lowered the pressure, there had not been a concomitant liability of its clotting in the presence of free oxygen. The physical necessity is therefore not an adaptation but a *sine qua non*. It is as it were a step on the physical ladder by which the mammals were enabled to come into existence and to build themselves up by new functions into still more complex structure.

Organisms and New Creation.—If this so far as it goes is a fair statement as regards evolutionary construction, we begin to see that each new organism is truly a new creation following afresh a definite physical, chemical, and colloidal path, plotted out by past evolutionary processes, which can be changed only very slowly. In considering the machinery by which these results are assured we have to rely for the most part on the labours of workers in the laboratory, and as this branch of experimental physiology is still in its infancy we cannot get all our equations properly developed in their proper places. But it may be affirmed with confidence that the results this study has produced fit in with the thesis which could be guessed a priori,

that continuously produced areas of relative activity, such as the axial gradients of Child, a great pioneer in this wild country, the functions of catalysts, the eduction of late and later secretions, the environmental influence of embryonic parts on other parts, the absorption of early organs, the transformation of others, and a thousand other facts, imply a true and continually new epigenesis, following mechanical, physical, chemical and, we may even say, endocrinal law. It is due to this that there are seen in various embryonic stages certain phenomena which have been held to support some law of pre-determination. The phenomena, however, which seem to support it are capable of a simpler and less illogical interpretation. What happens is merely forced physical, bio-chemical and mechanical formation, always repeated unless changed by other forces. Epigenesis is incompatible with any use of the word which implies the conception of genes, or definite germinal or chromosomatic morphallactic factors, which always or already existed in the germ. They are there no more than potentialities, subject to disturbance. Epigenesis means properly this orderly eduction, by the active factors of any stage in question, of further stages, which will again be repeated in new individuals so long as the total environment does not change.

Preformation and Chemo-differentiation.—How is it then that in some embryos we may find areas which when excised and replanted in an abnormal position still produce what they would have produced if left *in situ?* Examples are easy to find in the work of experimental physiologists. Here we must remember some of the basal facts of senescence and rejuvenescence, of differentiation and de-differentiation. There is a stage of animal development in which de-differentiation and re-differentiation can completely remould an organism. There are organisms such as the higher mammals in which de-differentiation and metaplastic or prosoplastic growths are comparatively

minimal, though in certain cases, as in malignant disease, normal processes are so exaggerated as to become abnormal and destructive. But ordinary repair, as of wounds becoming scars, is the most obvious and salient example of normal, or nearly normal, regrowth in most mammals, though it seems that the excised bladder in the dog and even in man can actually be regenerated if the trigone be left intact. We can infer with absolute certainty that at each successive stage in evolutionary development certain possibilities of regression are eliminated, though some may still remain. Not yet do we know enough to state how it is that what J. S. Huxley has named chemo-differentiation can thus reach to a point in which it must inevitably work itself out if not inhibited. But we may guess that when the animal's soma has reached a certain stage of decreasing slowness in further development, the early stages of active, or more active, metabolism cannot be resumed. This probably implies that if it were possible to increase metabolic activity in any given part at any given stage to an abnormal rate, we might see re-emerge lost potentialities of differentiation. Let us note as an analogy that in national growth its organized apparatus of offence and defence may crumble, decay, be disorganized, or de-differentiated, and then grow again when national life is so stressed as to raise metabolism, which implies that metabolic changes again occur that were before impossible. Anything which alters metabolism determines new or altered construction. Thus, if stresses alter endocrinal action and such alterations are, as many physicians believe, truly heritable, there is to hand a real machinery for heritable modification. Any such alteration is, therefore, not a surprise or a proof of any kind of pre-determination, except on the lines of forced chemical influences, which produce their effects unless negatived. A common example of such changes is that caused by the optic disc, which, wherever placed under the skin, results in the

transparency of the overlying epidermis. When in Central America I have frequently observed a curiously similar result seen when the jigger or chigoe (*Dermophilus penetrans*) burrows into and under the skin, usually somewhere in the foot. Over this tiny female flea, which rapidly enlarges into a big sac of eggs, the skin, however thick, invariably becomes so transparent as to suggest a blister. This, too, is chemo-differentiation of a definitely analogous but purely pathological order.

J. S. Huxley and the Human Foot.—It is curious to observe that much of the valuable research into the rôle of chemical differentiation has been done by those who, when regarding the adult organism as a whole, seem to regard the environment with contempt. For all their labours in the laboratory go to prove that when taking every part of the organism by itself, the remaining parts, that is, its proper environment, are its most powerful determinants until the whole organism functions in the external environment. I noted earlier that J. S. Huxley (1926, p. 278), when concerned in his generally accurate remarks on bone growth to draw conclusions which he considers militate against any form of Lamarckism, used the fact that the unused human foot has an architecture far more simian than the normal adult foot owing to that having undergone direct functional modification. Let me lay stress on his words " far more simian." Because further development takes place when the foot is used, the result, he thinks, cannot be due to inherited modifications. These functional changes lead to what he calls the finer architecture of the foot. I fail to see why the changed and stressed form should be called " finer." The embryonic foot retains signs of an arboreal life. These are " entirely removed by long years of walking." This is not true. Perhaps Huxley has never seen an Oriental boatman use his foot as a hand. All this is, however, beside the point. For the embryonic human foot is distinctly recognizable as human in all its later

stages. And all embryonic protophases change with the forced stresses of adult life. That the embryonic foot is more simian than the used foot and yet is human shows that small though recognizable advances have occurred by means of variation following a line indicated by past function. I can discern nothing in these facts negativing the continued inheritance of modifications, for we should expect what may be called an embryonic "lag." And, since economy in hypothesis is the very basis of scientific reasoning, it is far more economical to draw the conclusion that the unused embryonic foot, distinctly human though not of the adult and used type, is the result of past internal nutrition factors directed by function and inherited, than to infer that it is due to innumerable happy accidents, or to chance mutations. It is part of any reasonable scheme of Lamarckism, however considered, to take it for granted, not that functional changes are at once inherited up to the hilt, but that they so alter nutrition and blood-supply as to force a continual approximation to the perfect functioning form after exercise. If the Mendelian-Darwinians want "genes" they can have them, but when they claim all function as well and then deny that any appearance of inheritance is due to individual past functioning they reach the limits of credibility. The mutable colloidal systems of which life is composed must always vary as function rules the distribution of energy. The inertia of a moving body may be hard to overcome, but it may be slackened or changed in direction, and when the change happens it tends to continue in the new line until again deflected. To those unaccustomed to look on biological problems in such light as available knowledge affords, these remarks may seem idle, but chemists and physicists are not likely to remain orthodox in biology if they ever turn their attention to what is said to support the generally-accepted modern theory. They will surely see that life cannot be divided from its environment, and that to imagine

that its forces when steadily persistent have no effect is absurd. It is when geneticists make too much of their own case, and wholly misrepresent that of their opponents, that they gain, or seem to gain, the victory. J. S. Huxley says that the probability is that the broad outlines of an organism are predetermined inevitably but that details are, so to speak, finished off by function. True, and no Lamarckian would deny it. What he would say is that gradually the acquired temporarily or relatively permanent plan is added to, and that its additions come from functional actions slowly changing the energy distribution by use and disuse, alterations in the endocrines, in blood and lymph supply and so on, so that physical and chemical forces work out to their logical, somatic and inevitable conclusion. And this process, whether by addition or subtraction, is for ever in progress save when the organism and environment are static and "adapted." To such a state all life and all science may tend, even the science of biology in the hands of the orthodox.

Passing Simian Characteristics.—Before leaving the simian foot and function we may refer to Keith's very interesting views on the present human resemblances to passing simian characteristics as suggestive of some kind of preformation. He remarks that certain early simian embryonic features, which are changed in all mammals but man, remain in him as continued adult characteristics. Bolk of Amsterdam has shown that the downward trend of the front part of the base of the skull is found at an early stage in the development of all mammals but that as the face is formed in other mammals than man this bend is undone. This is held to support a form of preformation. What would a mere logician, without knowledge of any of the sciences, feel bound to infer? Let us imagine that he had at least heard of Wolff's Law? No doubt it will seem a large assumption, even a *circulus in definiendo*, to say that we can infer from the facts of growth in the embryo and after birth, when

function begins, that Wolff's Law can be so far extended as to cover an organism's past history. But since it is clear that the form, say, of the adult femur follows the principles Wolff enunciated, and that the embryonic femur has like arrangements of the trabeculæ, it is confusing the whole question to assume that it is not to similar antecedents that the whole set of historic phylogenetic results is due. Many, not saturated with preformationism, will find infinitely less difficulty, and certainly more economy, in the inference that each generation's changes in accordance with changing stresses alter the total antecedents which modify the embryonic bone of the following generation, than in any other possible explanation. For though it may be, and is, denied, those who deny it can at least understand what is meant by the statement, and I defy anyone to produce an equally clear preformationist statement which avoids mystery or, at the least, the employment of ambiguous terms. In turning Wolff's Law into a law of functional heredity and the gradual eduction of osteoblastic potentialities there is neither mystery nor ambiguity. Its sole difficulty lies in our present but surely passing ignorance of inherited hormonic direction of energy. But this kind of difficulty we can meet with in any problem and it need not deter us from assuming what affords a real and not a false explanation. By false explanation I mean one which interpolates at any stage some undefined and indefinable agency. I infer, then, that our mere logician would say at once that the forced functions of the later bone structure of the face in the lower mammals had so stressed the early structures that, in accordance with Wolff's principles, they changed to meet these new stresses on mechanical lines. He would add that the cranial bend in man still remained because these antecedent stresses had never affected him. He could go further and urge that there was no real reason why man might not have been subjected to them and still

have become Homo Sapiens, and if he had done so this cranial bend would have been undone in him. We have no right to appeal to any Cæsar of preformation if we can supply a reasonable set of mechanical causes. And let us note, as I believe was noted before, that Wolff builded better than he knew. It is not only bone but also muscle and every other tissue which respond, within the limits of its capacities and its stage of growth, to stresses which alter their formation in accordance with mechanical law. This fundamental property of protoplasm is the basis of "adaptation." Thus, when we consider man's lucky failure to specialize it is not hard to understand why he should preserve some or many of the unspecialized features of his ancestors.

Muscle Deployment.—A similar explanation may be given to certain interesting and very obscure problems in muscle formation. The *peroneus tertius*, in its full form, is a human characteristic. But there are signs of it, just where the believer in descent with modifications would naturally look for them, in the gorilla, the very foot of which great ape declares it to be on the way to the upright posture. For those who accept this view, and with it the fresh epigenetic "creation" of new features, must infer that constant, or very frequent, attempts of the simious creature who evolved into man, to keep his feet flat by lifting up the upper edge of the sole, so influenced postural habit and nutrition by a new function as to induce new muscle growth. Even now the gorilla, influenced in the same way, is going through a like early stage. Keith, however, seems to interpret the facts otherwise. They are due to a latent myoblastic tendency to give a better functional result. Provided that this is interpreted as meaning that at any given stage of evolutionary stress the myoblasts tend to deploy where they can live and continue their development as muscle-cells, there can be no objection to the statement. It can be read as a corollary

of an extended Wolff's Law, such as that suggested. But if the interpretation required has to fit in with the views of Huxley a latent myoblastic potency must be given an element of foreseen preformation. Keith's actual words are—" a latent tendency in the myoblasts of the interosseous muscle, to deploy and group themselves in a new way, one which gave a better functional result." I do not see how this can be understood except as a potentiality of the myoblasts to migrate into or be newly " created " in functionally stressed areas in which they can continue to employ their remaining powers of differentiation. But this is not any Huxleian latency, it is the normal and natural " behaviour " of the myoblasts in the presence of fresh stimuli. This by itself would ensure a better functional result, and until contradicted I shall believe that this was what was meant, and that Keith's teleology will easily bear an interpretation in consonance with behaviourism, and with the work of C. M. Child. It may be said here that this behaviour of myoblasts is also in accord with Hunter's " consciousness " of tissue, as I have interpreted it elsewhere. The myoblasts go where they can do best, or rather, they become muscle where the rhythmic stresses, which develop and differentiate them, naturally come into play. No doubt it is a pity to have to use the word " consciousness," with its cloud of misty connotations, but Hunter used the word because he had no other to help him in the explanation of tissue action. In a paper on John Hunter (Roberts, 1929) I endeavoured to put what he obviously meant into more modern language. He saw that the tissues, and he had to speak of tissues, since he knew nothing of cells as units or in groups, acted as if they knew their business And clearly they do act, just as we ourselves act, in accordance with the stimuli to which they are exposed. This, as I ventured to point out, will not seem so absurd if it is understood that in their lesser ambit they act with more

certainty than human beings in their larger, less definite and infinitely more changeable one. " What we need to do is to put our ' mind,' or cerebral registering and reacting system, in its proper and more humble place than that we mostly claim for it. We are not such intellectual beings as we suppose." (1929.) It may be added that the myoblasts and osteoblasts have more " sense " than we usually imagine, since they follow their simpler nature more inevitably. If, as in muscle migration, myoblasts move more mysteriously we have no right to impute the cause to any schematism but must merely assume that here, as elsewhere, we are still very ignorant.

CHAPTER NINE

OBSCURANTISM AND BIOLOGY

Religion and Science—Biology and Orthodoxy—Hybrids of Science and Religion—Science and Emotion—The "Soul" and Science—Science and Philosophy—"Mind" and Matter—Stages of Analysis—Renascence of Belief—"Naïve Realism" and Transcendentalism—Philosophy and the Task of Science—Haldane and Hippocrates—Science and Reality—The Value of Philosophers—Notions of Matter—Moral Status of Philosophers—Language of Science—Religions, Doctrine and Evolution—The Biochemics of Religion—Religion and Pathology—"Mentalism."

Religion and Science.—Until lately it has been the practice in all modern scientific work to avoid strictly any introduction of religious principles or even feelings. No doubt this has been because men of science never forget that religious bodies had always and in every way opposed the advance of science. There was no getting over history in that respect. Men in the laboratory not so long ago had to keep an eye on what the Churches would say of their work. They remembered the enforced secrecy of Copernicus, the humiliation of Galileo in Rome, and that of Buffon in face of the Sorbonne. Whatever form religion took, if in power it obstructed and hampered them when it was not able to imprison or burn them. They knew that the Churches would resort to ancient methods of suppression the moment they were able to do so. For the Churches have never tolerated what they can suppress and never will. They know that science has undermined their position. They see cherished opinions crumble. They note the spread of cynical indifference and recognize that

general indifference is more deadly for them than particular heresy. They still put its increase down to science. Even that enemy, the Higher Criticism, used cold, objective scientific methods. Yet now they begin again to hope. A reaction against pure scientific thinking is in full flood, and it is a pleasant change for them to observe learned professors and doctors succumb to the enchantment of words, just as some did of old. It is true that this " new thought " is not orthodox. But it is much for them to observe that it means a denial of the deadly objective methods which proved so destructive. In a little while they hope that " mind " will become " soul," and that the professors will return to the Church as humble penitents.

Biology and Orthodoxy.—It may be replied that this obvious reaction has nothing to do with biology. But biology is the science of life. Hitherto it has been the main task of a biologist to pursue his task of explanation of the phenomena of life by observation, experiment and clear thinking without any resort to any form of mysticism, however skilfully concealed. The seventeenth and eighteenth centuries have come into much disfavour with certain classes of so-called thinkers. They were materialistic centuries, so it is said. Perhaps they were. They were also the first centuries in which anything like cold reasoning came into favour. The coldest clearest thinker of the fifteenth and sixteenth centuries was Machiavelli. Whatever he meant by his conclusions his method was perfect. He divorced all the problems he treated from prejudice, good or bad. He was, considered merely as a logician, far beyond his time. Without knowing it the eighteenth century adopted his logic and cleared its mind of orthodox cant. When the Empire was established and Napoleon made his cynical compact with Rome, there was again a reaction. Lamarck showed forth the descent of man and doubtless laughed in his sleeve as he held up a tiny candle to the orthodox belief upon that subject. Again

we had a period of comparatively clear thinking. Darwin ignored all orthodox notions. His method, however inadequate, was purely mechanical. Since then, and perhaps at that very time, a reaction began. It seems that it is now at what clear thinkers may hope is the flood. The tried methods of science are being discarded by some who have lost patience and wish to settle things at once. They are not content to add a little. They fall back on unfounded assumptions and begin to deny causality. Instead of following the wise rules that no man of science should use words or symbols which cannot be defined, or believe anything is proved until it fits into the still incomplete web of knowledge, into the pattern and mosaic of science, they hint that what we seek to explain by patient work can be settled at once by the introduction of factors drawn from the rusty armoury of verbal metaphysics and theology. They allow their desires to overcome their very training, for it grows surer every day that what they aim at is the restoration of some kind of religious thought. They appeal to the public in popular essays. They even carry propaganda into the laboratory and hope to convert the patient and incredulous worker still untouched by such heresies.

Hybrids of Science and Religion.—It is the practice of modern scientific workers to treat each other, if not with real respect, at least with its appearance. This no doubt is an advance in civilization and much to be commended. But what self-control it takes may be learnt in private conversation when the present movement towards the production of infertile hybrids between religion and science is the subject of discussion. I have urged on some with a right to be heard that it was their duty to say what they really thought when men eminent in various branches of science took to these newer forms of obscurantism. They showed themselves reluctant to do so for many reasons not difficult to understand and some suggested that it would be better if the task were undertaken by one of more

independence while they preserved the serenity necessary for their work. This hint might have been lost on me if I had not happened in a lonely Exmoor village, which was spoken of in the chapter dealing with the Serpent's Fang, to come upon one of those notably common books nowadays made up of a series of discourses by several hands. It grew clear when reading it that the curious reaction seen of late to the savager forms and ritual of religion had not left some of the eminent untouched. It is true they went what would seem a very little way to the reverting theologian. But still, they clearly found it difficult to remain in silence, merely attending to their proper business upon their particular pinnacles of physics, astronomy, or biology, which stood above the clouds, and felt impelled to descend into the mists of religion and philosophy and other forms of verbal exercises which still command some popular respect. I saw authorities to whom one bows come fluttering from their perches to take hold of things with which they are unacquainted, and, like retired notabilities who carry their honours and accumulations to the City and come forth denuded of both, to talk at large not only of matters beyond them but beyond everyone. By these is meant those games and exercises with words which are called pure psychology and abstract philosophy. And, in every case, what is the reason? There can be little doubt left to us when we peruse *Science, Religion and Reality*, and find half a score of Cardinals, so to speak, who in solemn conclave give us almost openly their secret thoughts about immortality. For that is really what some of them mean when they talk about "mind," and try to make out that this portmanteau word, so convenient in common conversation as verbal shorthand, really means something consonant with but not analysable by science.

Science and Emotion.—Why, then, do these eminent men think as they seem to think? Not so much knowledge

of mankind in general is needed to assure us that the reasons they offer merely cover up the truth. They argue as they do because their emotions give them both arguments and words, with which they try to hide their doubts and discomforts. We need not deny their sincerity. We often note fervour in the speech of fools and we know that the wisest can be the victims of teaching they have long forgotten, which yet remains the present stock-in-trade of the pulpits. If this is so their science is unsound and their conclusions, when they reach beyond the measure and the scales, have no validity. For we can argue, and all men do at times argue, really more from the hidden premisses of emotion and desire than from what we can prove, and then our logic is but that of persuasion and often enough a vain self-persuasion at that.

For, concealed or unconcealed, in that major premiss their cerebral constitution, the result of heredity and instruction, lies the hidden desire of permanence. They cannot do without some kind of immortality. Without it their very life's work seems vain. It is not thus that the votary of pure science prepares himself for his task. The work before him is explanation by means of the explicable : he avoids words double or triple or even infinite in meaning, as he adds to that knowledge which, among all the " realities " which the human mind may conceive, has at least the basis of consonance with the conclusions of his fellows, those who are yet at work and those who laid the foundations of real science long ago.

It seems, therefore, impossible to avoid a few words with the new and at times rank growth of neo-vitalist shadowy deists which now afflicts science. It is true enough that those capable of defending their semi-spiritual prepossessions with any show of argument are few, but they are followed eagerly by that army of the ignorant which clamours to be reassured by some soulful professor that there is some sort of spirit immanent in all things, as

naked natural action seems, so one would think, indecent to their native instincts. For any to imagine that "brute matter" can rise to the height it attains in some of their fellows really afflicts them sorely, for have they not been assured that matter, which for them is but dust and ashes, is gross and ignoble and must so remain? Therefore they call upon their gods to reassure them lest they be argued into the belief that spirit, of which they know just as much as any bishop and no more, is finally non-existent, since it has at last suffered death in the laboratory. They abhor materialists and are much comforted to learn that great scientific authorities have really rescued religion, though it is in a very sad state after many experiences *in vitro*, and have also saved spirit, which henceforth, we may take it, is to be known as something which, though not exactly mind, is rather like mind and takes charge of an organism "as a whole," "if and when" delivered to its care.

The "Soul" and Science.—To be sure the salvors of the soul, for it is in truth the soul which they are concerned for, are not without support from some unexpected quarters. I find that Professor Eddington, who taught the curious as much of the Einstein theory as they are ever likely to assimilate, is more or less on their side. If his argument is summarized without any expectation of his agreement, it may be said that it depends, as so much has always depended, on the peculiar fact that the universe as we know it takes exactly the form that on investigation we find it in. Therefore it depends on us, on the nature of our "minds," and mind is a word he and no one else ever really defines. If, then, we were utterly different, and thought that two and two made five, the universe would be different. So far so good. The world of a mouse and a man *are* different worlds. But who ever said they were not? If man as a complex set of colloidal systems so reacts to the stimuli of his organic and inorganic surroundings

that he is compelled to conceive the whole system of stimuli as of some particular order, on what ground must we postulate that not he, but something vaguely descriptive of his reactions, is what reacts and keeps him going ?

Science and Philosophy.—A great mistake was made when the first instincts of scientific men to call general science " philosophy " was rejected and put aside with scorn by the metaphysicians when they jeered at the phrase " Philosophical Transactions." Scientific workers, being less skilled in controversy than the verbalists, accepted this too meekly. This has led them in many cases to accept the view that metaphysics has a claim to higher rank than physics. As a consequence metaphysics with its natural leaning to complete early explanations, which it acquired from religion and scholasticism, has affected very seriously many who might have been expected to escape metaphysical verbal delusions. So when physicists become metaphysicians they naturally assume the authority of their rank in their own science. Great men in what they know are held to be great men in what they do not and cannot know. We all want to make " the passage to physics " and must readily accept a helping hand when physicists offer it. But when instead of help we get obscurantism and a passage to some kind of theology we owe them no gratitude. The disregard of pure science in England is unhappy and disastrous. It can be remedied only by workers sticking to their work. To leave the laboratory bench and assume the mantle " star-enwrought " of the metaphysicians harms the whole cause of science and tends, when physicists attempt the impossible, to bring the highest and most inclusive science of all into the contempt with which the average man regards it. Perhaps the man in the street is, from his own point of view, justified in disregarding the work of the great physicists, since he has neither the power nor the desire to indulge in or to understand abstract reasoning. He has his own work to do in his own orbit and in the social

" fields of force." Unhappily too many workers in the great subordinate sciences are apt to feel the same as the unscientific. They should be encouraged to use physics far more than they do. Are they encouraged by what they see and hear from too many physicists? I think not. They begin to feel, they do actually feel, that their work stands in no need of correction or advice from a physicist who " commences " philosopher. The one justification possible for any philosophy of physics must be the proof that it must be taken into account by workers in the laboratory and even in the field. Common sense must be shown that it can be advantaged by uncommon sense. Bertrand Russell in *The Analysis of Matter* says: " The further science advances the more minute become the facts which it cannot assimilate. Common sense does well enough for the needs of a pre-industrial community but not for the construction of a dynamo or a wireless station. For there we have to advance to the standpoint of pre-relativity physics. Machines involving relativity physics do not yet exist, but presumably they will some day." This seems a frank admission, or at least an implicit admission, that physics may be judged by naïve realists by what it can do, though he adds, rightly enough of course, " if physics had to desert common sense that is no reason for finding fault with physics." However there happens to be more than one way of deserting common sense, and perhaps Russell's own use of the word " mind " may be one of them. For it is impossible not to regard the use of " mind " without strict definition as something in the nature of a concession to those who regard " consciousness " as an entity of sorts to be accepted at once without question. And Russell, who owns his obligations to Eddington, follows him in saying there may be limits to physical determination. " Thus we are forced to descend into the region of quantum transactions and desert the macroscopic level where statistical averages obtain." It is from this region that Eddington

draws doubtful encouragement for those who believe in free-will, understood as indetermination. It must in fairness be added that what Russell says may seem of less importance when we note (p. 393) his dictum: " Thus we cannot escape from the universality of physical causation." Such contradictions or apparent contradictions are of very little importance in such brilliant work. But Eddington remarks that there are entities which obey laws unknown in nature, and this introduces us to a new kind of metaphysics, to a new province of Nephelococcygia. We need not impute this saying to him as a crude denial of all science, but have to presume he is referring to quanta and the like, of which at present so little is known that any dogmatic statement about them is rash even for a physicist. He takes it that " mental " selection chooses and states what is to be looked on as a law of nature. But is it not perfectly clear that this very selection is an automatic process of his own cerebral organization to the complex problems represented by, and in, his subject-matter? Let the Professor try to make another selection and give us a new set of natural laws and see if he, his " mind " or his brain, will do it. Not thus can he or any of us get away from the forced path by which mankind has ever sought to simplify, and as it were make diagrammatic, the universe.

What is in fact the evidence for mind in itself? There is none whatever, nothing but dim surmise and the unconquerable hope of the religious. For consider that Eddington, who clearly is of this order, writes " there is nothing to prevent the assemblage of atoms forming the brain from being itself a thinking machine in virtue of that nature which physics leaves undetermined and undeterminable."

Now here we have a statement capable of analysis. I shall let the word " undeterminable " carry what significance it may to those who have observed that the determination of the yet undetermined is the great task of science. It will be seen later that Dr. Haldane practically agrees

with Professor Eddington when he, too, denies science by stating that organic co-ordination can never be explained. But, putting this aside, we may say that " there is nothing to prevent " is good. So is the " mobled queen." There is obviously nothing, according to physics, to prevent the brain atoms being something which has nothing to do with physics. We can agree cordially that no knowledge of physics prevents our imaginary brain atoms being black or blue or three-cornered. This thesis affords great ground for the action of any professor's imagination. But the mechanico-physical physiologist is left cold, for he must still presume that each cell, each cerebral neuron, is built in accordance with physical law, and it is in the interactions of these cells, in their functions, that he finds what we call thought, that is, the highly complicated conditioned reflexes which control behaviour. He leaves Professor Eddington to say if he pleases that there is something else in any engine but its energy and its working, but, not having any occasion for the afreet to do the work, he has no use for it whatever. How far Eddington goes may be judged from one isolated remark: " for the contact of matter and spirit is between brain and mind " ! The action of atoms and electrons and quanta may often be inscrutable and seem transcendental to those to whom transcendentalism appeals, but that is no argument to show that transcendental and inscrutable " mind " exists and is anything more than a descriptive word for reactions which are often difficult to analyse but for the most part are obvious enough. And in any case there is no onus on the mechanico-physical physiologist or philosopher to run ahead of science and, finding himself in difficulties, to call up gods and noumena. We may conclude that the necessity to have " mind " in the scheme at all is a very common human weakness and so far a denial of science. The first business of philosophy is economy, and that is why philosophy seems nowadays confused to those who ask words their meaning before they use them.

Pavlov is greater than Plato. If some of the others would but consider that what they call "mind" is in truth *function*, and therefore ethereal and passing enough in all conscience, they might even find it "spiritual" enough for them.

"*Mind*" *and Matter.*—That matter, still afflicted by the mouldy connotations implied by its theological opposition to spirit, should seem incapable of doing all that mind, soul and spirit do, and may yet be capable of higher things, is natural enough when we look into those primitive methods of thinking which still form the unacknowledged basis of thought in most people. These know nothing of the brain or its capacities. They cannot appreciate the meaning of a million, and the fact that some 9,200,000,000 neurons form the cortex of the brain is but a set of empty words. Yet what are the mathematical capacities of change and fresh linkage among these neurons and what do they become when we add the nerve dendrites with their multiplied and infinite potentialities? For though we are born into this life probably with all our neurons complete, since they are not a committee capable of adding to their number, these dendrites are clearly capable of growth and the formation of new synapses. What, indeed, is the neurological difference between the educated and the uneducated but their education and fresh linkages? What is the difference between the fresh "mind" and the aged and rigid "mind" but the capacity of forming new synapses, of new short circuits with a freeing of energy? Thus, and not with feeble words, can we explain the struggles of a Kepler among the complexities of the heavens and mathematical conics, and his sudden joy, or energy release from the gnawing anxieties of an apparently insoluble problem, when he found the ellipse satisfied himself and the obscurities of the planetary orbits. And thus we can also explain the collapse of thinking when it is said "we shall never know."

But, say the religious and the mystic, or the philosopher who has leant all his weight upon words, can we thus explain not scientific but religious phenomena? Well, alienists can tell us of strange mad exaltations of "spirit," and they can often cure them. Here they are the results of a diseased and disordered brain. Can we not equally explain the strange and wonderful exaltation of "spirit" which we find, however vaguely and unconvincingly expressed, in mysticism, in poetry, and even in the best and noblest theological thinking? I can find no reason for anyone thinking otherwise than a confirmed cerebral necessity some have for assuming personal permanence. The sane scientific view is that there is no high activity of man which cannot reasonably be explained as the result of neuronal action. When any result of such cerebral activity transcends, or seems to transcend, the bounds commonly placed on the powers of the brain, this merely means that it has constructed something false, however beautiful, or, as I prefer to believe, that the bounds of neurological actions are co-terminous with, and exactly the same as, the bounds of the "spirit," which is merely a cerebral hypothesis, wholly incapable of proof. Is this an exaltation of matter? I regard it as a legitimate statement, since it obviously avoids the confusion of double explanations. Even the philosopher and the theologian must practise economy.

I noticed some time ago with interest that J. S. Huxley defined a materialist as one who believes that matter is either the sole reality or that it has in some way generated mind as a sort of by-product, and so is "of more ultimate importance than mind." I am much inclined to think that most "materialists," unless they are pure metaphysicians for whom the scientific materialist has a well-founded contempt, would decline to attach any meaning whatever to the word "reality" the moment it was removed from the laboratory of weighing and measuring. This is, indeed, the

great distinction between new obscurantists and those who, taught by the experience of ages as well as their own instincts, simply have no use at all for words which cannot be satisfactorily defined. To such men " ultimate reality " is merely an empty phrase, so empty indeed that it can be filled up to order by every antagonist school of metaphysics.

Nor, if I am a materialist, do I believe that matter " generated " mind. For this implies that I believe in " mind " as some form of entity, which I cannot do. *Mind* as I conceive it means the reactions of the brain to the external and internal environment and means no more. Mind, soul, spirit, and consciousness as well, are therefore loose allied words which are useful in talk and literature for slightly different aspects of cerebral work. The assumption that they are more is at once unscientific and unphilosophical, and here we may distinguish a sane logical philosophy from metaphysics because that is always a personal multiplication of unnecessary hypotheses. The very fact that many deny the existence of any of these " things " might surely suggest that Spinoza was right when he said we did not know what " body " (corpus) could do or could not do. It is only those with an incurably metaphysical or religious cerebral organization who cannot see that we must wholly exhaust the possibilities of matter, body, and brain before we call in despair on hypothetic verbal spectres, even to salve, with the help of Eddington and a probably unwilling Planck, the sacred ghost of free-will.

Stages of Analysis.—It must be perfectly obvious that a great deal of controversy on these obscure questions depends on the stage of analysis at which we prefer to stop. We can analyse until we are as blind to anything as if we looked into a microscope when it is tried beyond all power of definition. Is there in fact what might be called a right or reasonable stage ? Again it may be said that we cannot in reason go very far beyond the region where words mean

the same thing to all of us. The philosopher Sir William Hamilton said that new words were fortresses with which we held down freshly conquered territory. When those who go beyond a common vocabulary find new intelligible and exact words we shall be able to follow them. Till they do so anything they say is but suggestion. The ordinary man of science and the great man of science may legitimately use suggestion. But suggestion cannot be tortured into proof. All the greatest scientific work has been done by great naïve realists, those who looked on what they saw as realities : those who used logic, verbal or mathematical, as simply as the child who multiplies two by two. If this is so there is always a temporarily correct stage to which analysis can be carried. We must stop when words have more fluff about them than meaning, when they require explanation by the use of words still less certain in connotation.

It is easy to understand that this method of thinking must be repugnant to many. As a rule men are convinced that they have reached a high state of civilization and they therefore find it difficult to imagine that their religious and philosophical beliefs are but little removed from those accepted and sanctified in ancient or primitive societies. We are not seldom confronted in theology with discourses based upon savage creeds very slightly transformed, and the less reason there seems to be for accepting them the more they appear to excite the ardour of their votaries. I am by no means inclined to deal with these aspects of faith, by which I understand a capacity for believing statements that no one, unmarked by prejudiced instruction in childhood, could accept for a moment, and prefer while avoiding them to say that the average subjective psychologist might, not without reason, also be classed among those who begin all discussion with a background of " mental " beliefs they cannot, and often dare not, analyse. But when we consider that there are still many who believe firmly

in what Cardinal Newman said he believed, owing to his inner convictions, in spite of the overwhelming evidence to the contrary that the world offered him and offers us, it is not so surprising that most people should have acquired a firm belief in the independent existence of mind, spirit, and soul. We cannot expect them to get away from their early education, and if they become psychologists of the older school " mind and consciousness " form their stock-in-trade. We should, however, see without much difficulty that as soon as some totally imponderable entity is imported into pure science, science is no longer pure but a semi-mystical hodge-podge. These later forms of Neo-Mysticism render their votaries suspect. However good and even brilliant their work may be it is almost impossible not to be sure that some at least of their conclusions are illegitimately reached.

Renascence of " Belief."—Since the war, and great wars are often followed by a strange renascence of barbaric belief, the activity of the churches might lead those who know no better into the belief that these primitive disorders of the pure intellect have spread very widely among scientific men. Scarcely a week passes without some decanal or episcopal pronouncement that no one is now a " materialist," and that the notion of a mechanico-physical explanation of life and the universe has been given up. As it seldom happens that scientific workers have any time or taste for contradicting every foolish thing in the newspapers they naturally say nothing and get on with the task in front of them. And their task is usually to add just a little more to that very mechanico-physical explanation which they are said to have discarded. Perhaps it is a pity that these men should be so silent, for if the celebrated Bellman was right in stating that what he said three times was true, those uninstructed in the nature of the general scientific intellect may end in believing what they are told thousands of times by very foolish people. For this reason we may well be

glad of Sir Arthur Keith's late pronouncement which, no doubt, was a sad blow to those who like to believe that Sir Oliver Lodge represents the whole scientific world. And here the critical and curious may be referred to this great physicist's late paper in *Nature*, in which he touches on biology and extracts variations from the ether. He says: "My suggestion is that life is something which primarily exists in space though we only know of it when it is associated with and displayed by matter and I ask biologists to consider whether they could not, as a working hypothesis, begin to contemplate life as something existing in space, as in a sort of infinite reservoir, out of which it could, under appropriate stimulus, enter into association with molecules of sufficient complexity to enable it to catch hold and become as it were incarnate." He goes on to infer a concealed mechanism, not of a perceptibly natural kind, actuated by something at present unknown. The concealed powers have put the organism together in a special form, out of such materials as came to hand. However, when the machine gets out of order the controlling powers cannot display themselves.

Is this surprising when his colleague, Professor Ambrose Fleming, accepts with ardour miracles, once believed by the child-like to be a proof of divinity, though they are rightly discarded by modernist religious authority as unlikely and irrelevant.

"Naïve Realism" and Transcendentalism.—This last passage may seem irrelevant. I do not think it is. Let people say what they will the chief enemy of science has always been religion and it always will be so long as it palters with its practical and social work and makes for transcendentalism of the theological kind. Nowadays when the Churches are fighting for their authority and their lives, all the time in deadly fear of science to which they hold out appealing hands for help, it seems a most disastrous thing that they should get it from those who know quite

well that a dominant church would be just as obscurantist as all churches have always been when in power. How much help they do as a matter of fact get may be seen when any man with authority in science says something which may or might mean, or can be tortured into meaning, what they want to hear. It may no doubt be said that I have no authority whatever to speak on behalf of the general body of scientific workers. That is true enough. But that does not deter me from saying that there are enormous numbers of such workers who feel exactly as I do myself. They may be poor naïve realists, but they know that these philosophers, whose metaphysical tendencies they detest, are also naïve realists at the bench, in their bath, their bed and at breakfast. They do good work as such and then break out into a rash of metaphysics and undo half the work they might have done. Because during difficult and arduous enterprise upon the very verge of the unknown they find the air almost too rarefied to breathe, they have no right to erect their very natural and unblameworthy ignorance into a "proof" that they have somehow reached a region in which law fails. To do that was once the mark of ignorance and presumption, which in other times put Galileo in prison and burnt Bruno. There is no need for physicists to propagate waves of obscurantism. We have enough of it without them.

Philosophy and the Task of Science.—In addition to what may be found in the book criticized something may be said of another, which I hear "well spoken of." This is Dr. Haldane's "*The Sciences and Philosophy.*" None acquainted with Haldane's physiological work have anything but respect for it, though it has been a common-place for many years among scientific workers that its general value has been greatly lessened in effect by his peculiar addiction to forms of philosophy that none can call scientific. When it is found that the conclusions that seem dearest to a worker in science are not based upon scientific reasoning but are

distinctly to be traced to emotion, it is but natural for the incredulous and merely brutal thinker to regard his best work with suspicion. This is due to the fact that Haldane quite obviously has little respect for much work done by his fellows. When he writes: "What we cannot trace, however, is any progress towards a comprehensive physico-chemical interpretation of biological observation," some must be tempted to ask whether his philosophical prepossessions have not prevented him observing the real, though slow, advances made towards such an interpretation, many of them made by himself. But what can any unprejudiced thinker say of his statement that nothing known to science can hope to explain the co-ordinated activity of organisms? Is it then true that all biology is vain? What is it in the end but this interpretation that men of science are seeking? To discover how the body is built and how it acts, and by what mechanism co-ordination begins, improves and is more or less perfected, has been the task of science since the time when by experiment alien and imaginary agents were one by one put aside. Haldane states that "physiology has thus failed to give to scientific medicine the help which it ought to give, since it has nothing coherent to say of the co-ordination which is the distinguishing feature of life or about the natural processes of resistance to disturbance and recovery from it." A more astonishing statement has never been made by anyone presumably acquainted with physiology as a science and medicine as an art. What would Starling and Bayliss have said of this? What would the endocrinologists and immunologists say? I suggest that Pavlov might also make some remarks relevant to the question. Has nothing been done of late years on the autonomy within limits of the various somatic tissues and organs, nothing on the way in which they protect "themselves" and resist and partly conform to the influence of the excretions and secretions of neighbouring or distant organs? Are not the advances made in immunology

advances in the understanding of co-ordination? Co-ordination is the related working of the whole body, and no branch of biology and all its ancillary sciences but has increased our knowledge of it since Sydenham took physicians at least part of the way back to Hippocrates and more especially since John and William Hunter did their work. So if we do not yet know enough of co-ordination to please a philosopher who desires, naturally but vainly, an early and complete explanation of it, we can rest assured that this, the greatest end both of theoretic and practical science, is not so far beyond investigation as to make despair reign in the laboratory. It has sometimes been made a complaint against Darwin that he had too little to say of correlation and co-ordination. It may be urged on his behalf that however much he believed that he had solved the main difficulties of a mechanical evolution, he could not, and assuredly did not, think that such a solution cleared up everything. The more he believed in his own method and results the more easy he must have imagined the work he left to others. In his time he was clearly justified, but with the added machinery just indicated and some knowledge of pathology and the obvious inference that evolution is always going on in a perpetual disorder he would have been in a better position to attempt a far more complete explanation not only of correlation and co-ordination but of the painful imperfections of both.

Haldane and Hippocrates.—Haldane's view that Hippocrates saw clearly that the mechanic interpretation of life was impossible cannot be defended, but even if that was the opinion of the Father of Medicine, what he thought is clearly irrelevant to modern work on the way "one part sympathizes with every other part." No doubt he held, very lightly one would think when reading his work, some of the vague philosophy of his time, but in its essence the doctrine of the Pneuma was materialistic and his insistence that all diseases are natural takes us

up to John Hunter in one leap. If Grote's remark that Hippocrates regarded all phenomena as at once divine and scientifically determinable is as true as I believe it to be, he is put out of court as a witness for any modern brand of semi-mystical thought. Those who doubt may consult again what he says of epilepsy, the Sacred Disease. Certainly we have no right to demand of Hippocrates a complete theory of energy and protoplasm and the way to co-ordination. It is possible that not a few surgeons might still read, in the added light of modern practice, what he has to say of fractures, but we cannot go back to him for things which were as much out of his ambit as out of his knowledge.

Science and "Reality."—When we examine closely Haldane's real meaning it is clear that his conclusions are not even remotely scientific. They are the natural offspring of his native passions for the "Real" and "Reality." He says: "The real universe is the spiritual universe, in which spiritual values count for everything." Those who, like myself, have in earlier years wasted much time in trying to find out what is meant when philosophers talk of the Real and Reality, and have come to the despairing conclusions that they mean exactly what any philosopher's particular cerebral organization compels him to wish them to mean, will naturally enough turn away from such verbal exercises and go back to their work at the bench or by the bedside with a consciousness of having read something irrelevant to their labours. And as I have used the word "consciousness," though merely as a literary symbol, I may remark that when Haldane complains that mechanists have made no real attempt to explain consciousness, the lamentable truth may be that they agree with some modern behaviourists in thinking no such thing exists, and that what we mistake for it is nothing but irradiation spreading from a cerebral centre over related parts of the brain, a kind of "massive sensation" as Alexander Bain might

have put it, as he was one of the first to apply physiology to the elucidation of " mental " states.

It is impossible to resist the temptation to remark that the only philosophy which is of any use in the world is what philosophers esteem no philosophy at all. The whole work of the world, or at least that outside the moss-grown cloister of scholasticism and the dimly lighted study of metaphysics, has been done and is being done and will continue to be done, on the lines of Naïve Realism. In that the physician and the peasant, the engineer and the statesman, and all in touch with the reality that affects us are essentially agreed. They may even combine it with a simple monism or some harmless relics of a religion which is not less reasonable than the substitutes offered them in exchange by doctors of philosophy or of philosophy and science curiously mixed.

The Value of Philosophers.—It must, however, in fairness be owned that philosophers after all do fulfil one function. The naïve workers in science strive to build a causeway over the mighty morass of the unknown. They work patiently and with ceaseless hope, and even as they strive they know too well that it cannot be theirs to achieve finality or complete understanding. And while they labour among the waters in mist and darkness they see come among them the strange fanatic figures of philosophers, each carrying a dim candle, who call out aloud, " This, this is the way ! " And then there comes a cry of despair as the dim lantern sinks and is seen no more. For then those who are at work do at least know that which is not the way. It should, however, be added in fairness that among philosophers we can discover such as Kant, who possessed the scientific imagination in a very high degree, and should be credited with the first clear basis for conceptions which, when carried somewhat further, have made the reputation of others. These, perhaps, did not give him all the credit he deserved. It would be

interesting to consider what his views might have been if he had been a student of physiology as well as physics. It may be said that he was as much destructive as constructive, and it cannot be denied that the world owes more to destructive philosophers than to those who have erected delicate and elaborate schemes of final explanation or have resorted in despair to the defence of some religious or pseudo-religious conception of the origin and conception of the universe. But any real absorption in philosophy or metaphysics is for a worker in science not only useless but dangerous. Such speculations not only do not employ his tests of relative truth but they practically deny the validity of his method by which any conclusion is finally tested by its consonance with ascertained fact. It may seem narrow to seek to debar the eager young student from the verbal delights of abstract philosophy as some relief from real work, but he should consider it rather curiously than with much care lest the charms of such reasoning should lay hold of him to the detriment of his real task. If he succeeds in thus detaching himself he will find almost every kind of metaphysical study very useful mental gymnastics, while the study of man's inborn errors of mental metabolism will surely afford him considerable logical pleasure. So far, indeed, abstract philosophy will do him no more harm than any other form of imaginative literature and may indeed do good. It may even become an amusing means of gaining that recreative freedom which preserves workers from the staleness of intellect likely to supervene on unrestricted and continuous absorption in a single line of thought.

Notions of Matter.—All who have pursued the study of religions by the historical and comparative methods are of course aware that every advance towards reason in their fundamental hypotheses has been marked by a progressive relegation of the imaginary creator or creators of the world to loftier and more inaccessible regions and the imputation

of those offices, of which he has been summarily deprived, to natural forces. It is not here proposed to inquire whether the current religion owes its survival to the peculiar compromises on which it is founded, compromises which, while respecting this aloofness of the prime deity, maintain two wholly human elements of which different uses are made by the two great sects. For it is possibly due to this supposed intimate relation to humanity that the notions of deity among Western races are distinctly less advanced philosophically than those among Oriental races who have so refined them that they often seem even less than spiritual pantheism, while in some cases even their ceremony and ritual appear to the ordinary observer as a kind of Ark of the Covenant with the contents abstracted. Possibly it may be said that when Orientals take to science they are possibly less hampered by our curiously derived notions of the inferiority of matter and its supposed lack of high potentialities, for many of them, by other paths than those followed by our greater physicists, have reached a conception of matter which is untainted by any simple and gross verbal materiality. It therefore seems as if we in the West are in some ways distinctly more childishly pagan than they. For among our intellectual workers we find those who cannot rise to the elegance and purity of the mechanico-physical explanation but are forced, as it seems constitutionally rather than intellectually, to retain at least the vague shadow of a directing deity to which they give the etiolated title of "mind." It is little wonder that those physically incapable of a pure scientific process *in vacuo*, however good the work they may do *in vitro*, should thus tend to retain the purely vulgar notion of mechanism as something, in some curious way, essentially "low." For to discover the mechanism of any process is but to arrange in due order the steps in which it occurs. It must be left to those singularly endowed constitutions to say that the marvellous manner in which natural

processes are seen to work, when we thus explain or arrange them, is of a lower order than those of so-called spiritual phenomena, which ex-hypothesis must defy all explanations until they at last find their place in a sound physiology which leaves all semi-savage figments to the metaphysician on psychologist. We recognize at last that all brands of Neo-Vitalism, Semi-Vitalism, are but dogma and superstition, writ very small indeed. And it begins to look as if we should presently import new superstitions from the land of Quanta.

Moral Status of Philosophers.—It may be of some interest to students of what in common parlance we do call the human mind, if we inquire how it comes about that those who show themselves incapable of the clear thinking which science, whether in theory or practice, demands with greater insistence than any other subject, are invariably convinced of their essential superiority over their opponents. It would seem that this sense of superiority is founded upon other beliefs which have as little foundation as their theories of a semi-dissolved mentality capable, on occasion, of interfering even with statistical physics. Judging them by analogy with theological writers, a comparison they challenge, the retention of "mind" as an integral factor of evolution seems to give them a sense of moral status which no monist of the mechanico-physical order can hope to possess. For do they not thus retain a clue to the explanation of the loftiest faculties and emotions of mankind, conscience, pity, imagination and reverence, all of which are neither known nor nascent in the lowlier brethren of mankind? It is true that no critic of this moral attitude can hope to elucidate the complex processes by which the conclusions of these students and professors have finally been reached. Once more we perhaps appeal to Cardinal Newman to explain the inexplicable. Against all observation of his own and against obvious logical argument he found himself by an intense internal conviction bound

to believe what he had learnt to believe through ten thousand nameless influences. The Cardinal had, among illogical men, the reputation of a logician. If we take that from him we may at least give him the credit of explaining how and why men came to believe with fervour that which no reason can defend. It is at any rate perfectly clear that the Neo-Vitalist of any order "cannot help it."

Language of Science.—I am moved at the end of this slight exposition of misled brilliancy to refer once again to one of my favourite authors. Probably Sir Henry Maine's "spirit" would be surprised to think he had written anything vital to pure science. And yet when he remarks that the human mind has never really grappled with any subject until it was provided with an armament of appropriate language and logic I am at once stayed from thinking immediately of the subjects to which he referred, subjects for which the juristic apparatus of Roman Law had unconsciously prepared a man of experience. For instead of turning at once to metaphysics and theology, I thought of the Russian physiologist Pavlov and the work he has done for science during many arduous and fruitful years. It is, or should be, a matter of common knowledge that he was moved to make his researches into Conditioned Reflexes by the absurdity of using in pure science the vague and obscure terminology of the psychologists, philosophers and theologians. As remarked earlier, I was the more interested in these researches because, long before hearing of them, I had run against the same difficulty and, in an unpublished paper, had worked out what I then termed "reflex cerebral arcs," for which I should now naturally substitute the phrase that Pavlov has rendered famous, not by a piece of mere theoretical reasoning like my own, but by prolonged, exhaustive, and convincing experiment. It is to these consequences of Pavlov's work that Maine's acute aphorism may be applied. Here at last we have the

beginnings, to say the least of it, of a language which will be devoid of the fuliginous obscurity which characterizes any tongue when it attempts to deal with the forms of our higher cerebral activities. In a little time we may find that words which mean just what any writer would have them mean are at last relegated to the obscure and foggy theological region in which they often had their birth. And those who have recognized the extraordinary and disastrous powers with which mere words so often clothe themselves will rejoice to see the last elements of magic removed from the vocabulary with which men of science have to do their work. They will also recognize the reason for the bitterness which the very name of Pavlov seems to give rise to in many.

Religions, Doctrine and Evolution.—Many may think such a discussion is properly outside the plan of a book mainly devoted to pure biological questions. I regard it as strictly relevant and as necessary as relevant. Old popular beliefs expressed in current semi-theological language long retain an apparent vitality which is a mere sham of actual life. As law must always lag behind the morality of a state's best citizens, so must all religion, and all verbal psychology, which cannot escape the taint incurred by sharing theological language, lag behind advancing knowledge. As a result, which remains long hidden, the disbelief which popular preachers, not all of them theologians, deplore in the pulpit and on the platform, is infinitely greater and more widespread than is known to any but the most intelligent of them. Their doctrines and their very language are regarded with peculiar but silent contempt. This silence is due to many factors, but the two main ones are undoubtedly indifference and traditional dislike to incurring unpopularity by the expression of views which appear to contradict current morality but really do nothing of the sort. And unless reasons have made him violently anti-clerical, it may be

that the intelligent indifferentist of modern times is inclined to look with interest on the struggles of ancient creeds to retain their position. So some, though with more sorrow, may regard the inevitable fate of those examples of the African fauna which linger long and are yet doomed to perish. The general laws of evolution govern not only the origin of species and genera with all their variations in growth and growth mechanism as well as their ultimate disappearance, but also the origin, growth, and final fate of faiths. No biologist who has ever spared the time to glance over doctrinal and philosophic history but must have been struck by finding there all the phenomena of growth, variation, and " the struggle of the parts " he is familiar with in the laboratory or the field. The decay of direct creation into the faint shadow of " mind " working in and with matter, is in fact strictly analogous with prolonged evolutionary degeneration. The result of its final disappearance from science, which it has so long hampered, will be a highly advantageous variation. It is a pity we have no one like Thomas Huxley to " unleash his war-dogs on the ' paper philosophers ' who now flood the pages of scientific journals and on the literary men who now pass glib and complacent judgments on Darwin." (A. Keith, 1927.)

The Bio-Chemics of Religion.—Now the book with which I have dealt was edited by Professor Joseph Needham, who as a student of bio-chemistry is too well known to need introduction. But though he defends mechanistic biology in what at a first glance might seem an apparently satisfactory manner, his super-added conclusions in a remarkable section entitled " Constructive Thoughts " let the whole religious cat out of the bag. " Life in all its forms is the phenomenal disturbance in the world of matter and energy when mind comes into existence. Living matter is the outward and visible sign of the presence of mind, the splash made by the entry of mental existence

into the sea of inert matter." My own " crude and gross " reaction to this, a repetition of what was said long ago by Bishop Berkeley, is that such a statement means absolutely nothing, unless we get " mind," as divorced from matter, given a clear definition. I should therefore have ignored what cannot be looked on as reasoning but only as an assumption of the sort familiar to us in religious and so-called philosophical discussions. But when I find that J. T. Cunningham in his latest work *Modern Biology* is moved to condemn Needham for his extreme mechanistic views of animal construction it seems that something more may be said. For Cunningham owns to being a vitalist of sorts, as he says living organisms are essentially different from inorganic non-living mechanisms. In common parlance that is so. In the same way we make all sorts of rough distinctions. But the whole trend of modern scientific discovery is the continued relegation of anything but recognizable factors into the void and inane in which gibber all the rude spiritual influences which represent the early hypotheses of our remote ancestors. Cunningham himself owns to believing that in some forms the " ego " is not " statistically determined," and therefore that man is not a *passive* spectator of automatic processes. I have italicized " passive " as a totally unjustifiable word, for whether we are, as most scientific observers believe, determined in all our actions, the essence of action is activity. We may suspect that Cunningham has been influenced by Eddington.

As said before, the view that our world is the world as we happen to see it, and that therefore there is nothing to prove it exists as we seem to think, is idle and otiose. The world as a kitten or a crocodile sees it is the kitten's or the crocodile's world. Ours is the world we live in: it is the only world we can hope to understand, and when we reflect on the enormous amount of human energy wasted in efforts to explain it as some totally different

kind of world, the worker who is not the victim of ancient delusions may well look on these philosophies as sheer emptiness and vanity. Whether our explanations are valid or not for some imaginary intelligence is irrelevant to the work before us.

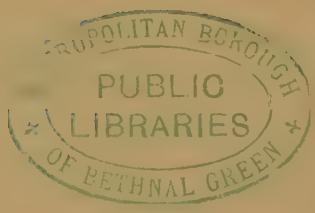
Religion and Pathology.—But, indeed, we may sum up what has been said of *Science, Religion and Reality* by remarking that its editor left it to a fairly "modernistic" exponent of religions to twist its varied threads into a rope with which to throttle the mechanico-physical naturalists. The Dean of St. Paul's by patient perseverance has so succeeded in capturing newspaper readers, who naturally admire his pleasantly discursive intellect, that he may now well be called our Standard Philosopher. I am little concerned to criticize his conclusions. He is naturally and obviously bound to come in the end into regions in which science has but one concern. This is, indeed, a remarkably interesting area which yet remains to be properly explored. Alienists have for long studied the grosser aberrations of mental, or cerebral, disorders, and have come to the conclusion that in them the victims reason naturally and quite logically from the premisses which seem best to fit in with and to explain their particular condition. It is also understood by anthropologists that the reasoning of primitive human beings is usually sound so far as it goes, if no examination of their major and minor premisses is permitted. In the natively religious there must always be something which is beyond direct argument. That any temporarily final hypothesis of this order is gradually destroyed by science and the general increase of knowledge we know, but so long as it is held it is held with something of the tenacity which characterizes those who, owing to stress or some somatic failure, have lost touch with pure reason. It has been remarked that as there is no state of health which cannot be found to shade off by imperceptible degrees into its pathological opposite,

so, together with most forms of mental, or cerebral, health, there are to be found states which form a most interesting class of borderland cases. This region may finally be explored by experimental psychologists and endocrinologists acting together, and when it is geographically delimitated, we shall probably discover that religious philosophers, whose final test of what it pleases them to call "reality" is some inherited or acquired verbal prejudice, are less to be trusted than the scientific worker who tests his conclusions by comparing them with the weighed and measured results of his unwearied fellows, reached through long generations of patiently verified labours.

For even those men of science who cannot wholly get clear of prejudice and an unhappy predilection for solving the unsolvable should leave a residuum of valuable work, which, being founded on observation and experiment, can take little harm from mere opinion about ultimate things. If indeed they find real consolation in their philosophy none need urge them to reject it. For it seems little understood, even by those most devoted to a providential scheme of the universe, that nothing in science prevents them from believing, if they will, that all the necessary means for continuance and immortality may be externally super-imposed on them after the clearest proof that the "mind" and body of man are wholly the results of a mechanico-physical apparatus. Science does not ask whence this machinery might come. It is scarcely likely to get beyond energy.

"*Mentalism.*"—I trust that nothing said in this section will be considered in any way disrespectful of the great physicists mentioned in it. A consistent and lifelong interest in their work, even though that interest is unsupported by a proper knowledge of mathematics, from the study of which I was most unwillingly rent at an unduly early age, may be held to be an excuse for criticism

when they leave physics for metaphysics. If I had observed that any of their eminent colleagues, and assuredly there are many, who do not agree with them, had undertaken the task, no one less well-equipped would have felt impelled to its execution. But when what I venture to call "mentalism" as distinct from vitalism and a dim deism, is called in to redress the balance of the old and the new physics, it is difficult to say that silence is justifiable. For everything of an obscurantist nature emanating from such sources is but too eagerly seized upon by those to whom science is really hateful, those who haunt the cloisters and catacombs of dead and dying beliefs, and kneel in the penumbra of a savage paganism.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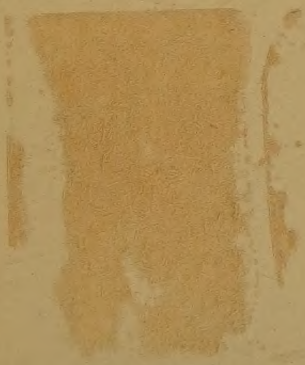

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